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AT the meeting held on October 26th, 1911, the following Demonstrations were given:

## A NEW FORM OF VERNIER MICROMETER.

BY MR. J. W. GORDON,

AND

## A NEW FORM OF REFLECTOR FOR ILLUMINATING FLAT SURFACES.

BY MESSRS. VAL. H. MACKINNEY AND
S. D. CHALMERS.

# ON THE POLARISATION OF LIGHT.

### Demonstration by Professor Silvanus Thompson, F.R.S.

*November 16th, 1911.*

Professor Thompson explained that he was about to give not a lecture but a demonstration in three parts. The first was to be an exhibition of polariscopic objects in parallel and convergent light. The second part related to improvements in the design of polarising prisms. In the third part he would demonstrate some recent work undertaken by him in conjunction with his colleague, Professor Coker, in the construction of improved polarising apparatus for use in engineering research—for examining specimens on a relatively large scale, such as 10 by 10 inches. For that part of the work, the specimens to be examined were transparent structures in celluloid or xylonite, which were examined in a condition of strain, so as to investigate optically the stresses that occurred in beams and other engineering structures.

Although not intending to give a lecture, Professor Thompson said that he would preface the demonstration with a few introductory remarks. He then briefly explained the polarisation of light, with the aid of mechanical illustrations, and with special reference to polarisation by reflection from black glass and by double refraction through Iceland spar. The opticians' tourmaline forceps was also mentioned, and an explanation given of chromatic polarisation by means of which it is possible to test the uniformity of thickness of a thin plate, of such a material as mica, and also to bring out by variations of colour, peculiarities of structure that are invisible by ordinary light.

The lantern was then brought into use, and a large number of remarkably fine crystalline specimens were projected by parallel plane polarised light, using the bright and dark fields to show complementary colours. These beautiful effects were much applauded by the audience, and must have been highly instructive to those who have not had the advantage of working with expensive apparatus. Amongst the objects exhibited were a series of amethystine quartzes, exhibiting successively the transition from simple quartz crystal to true amethystine structure. Some samples of toughened glass, showing coloured rings and a black cross, were exhibited, and Professor Thompson explained that the black (or white) cross traversing the coloured rings was indicative of a radial structure. Among the specimens shown was the crystalline lens from the eye of a fish, which had been put into a state of strain by the process of drying. There were also a pair of stepped series of mica plates, each showing singly the same series of colours, but when crossed giving a chess board pattern of colours, traversed by a diagonal row of black squares, wherever neutralisation occurred by the crossing of equal thicknesses at right angles. When superposed in reversed position (so as to equalise the thickness throughout), the step-wedges showed a perfectly uniform tint. A chilled-glass disc showed the glack cross characterising an invisible radial structure developed by the internal stresses resulting from severe contraction of the peripheral parts upon the interior.

The lecturer mentioned that Mr. Mackinney had just brought him some specimens of Kryptok fused lenses; and he proceeded to put them to the test of the polariscope lantern. The projection on the screen gave only a faint indication of a black cross, showing that these examples of different glasses fused together had been very successfully annealed.

Changing to the apparatus for projection by converging light, Professor Thompson demonstrated the characteristics of axis cut quartz. In this case there was no radial structure; but as the light crossed the slice of quartz in a converging beam, the thickness which the rays had to travel was a minimum for the central ray, and greater for the rays that came through obliquely. Referring to the quartz lens-tester or tourmaline forceps, he pointed out that it was practically converging light that was used in the application of the tourmalines, as light reached the eye of the observer from points in the sky extending over a wide angle.

Professor Thompson then exhibited a number of models of polarising prisms, and explained the construction of the Nicol prism, and of improved types due to various persons, including Mr. Ahrens and himself. Professor Thompson mentioned that he owned a prism which was cut by Nicol himself at the age of 79, and had been given to him by the late Mr. J. M. Bryson, optician, of Edinburgh. The speaker then spoke of a modification due to Hartnack, a German optician, who used linseed oil in the place of Canada balsam, placing the film at right angles to the optical axis of the spar. The speaker remarked that he had possessed one of these prisms, but lent it to a friend about 12 years ago, and it had not yet been returned. The use of an air-film, as suggested by Foucault, permitted a saving of spar, but caused a considerable narrowing of the field. Professor Thompson then explained an improvement of his own on Hartnack's prism, which had the advantage of permitting the spar to be cut with less waste. One of these prisms was made for him by Mr. Ahrens in 1880. His next idea for saving spar was to make the prism partly of glass; and that notion had lately been revived, using for the purpose one of the newer kinds of glass, having a more suitable refractive index. Mr. Ahrens's special method of cutting the crystals of spar so as to obtain wide-angled polarising prisms and double-image prisms, was explained by a model. He had experimented with poppy oil and balsam of copaiba for the internal film; also cutting the Nicol in the reverse way from that usual, and transposing the parts. In all, he referred to 12 different plans that he had evolved for the improvement of Nicol prisms, and mentioned that spectro-polariscopic prisms had been made also. After showing a number of different monaxial and biaxial crystals, Professor Thompson said that mica was biaxial, but in some instances came very near to the monaxial form of crystallisation; and this fact was illustrated on the screen by a series of specimens of mica from different parts of the world.

Professor Thompson then passed on to his experiments on optical stress in transparent materials under strain. For these he had had made a large polarising apparatus on the reflecting principle, using as polariser a sheet of blackened glass, about 11 inches by 24, set at the polarising angle, and uniformly illuminated by a bank of incandescent lamps behind a screen of ground glass. Binocular analysers similar to those which used to be used for the Anderton stereoscopic lantern pictures, were handed round among the audience. Professor Coker's experiments with celluloid structures were then shown, the black cross being got rid of by interposing quarter-wave plates on each side of the object. Thanks to the enterprise of Messrs. F. Wiggins & Son, very accurate quarter-wave plates had been made in mica, some 10 inches and some 12 inches square, and even, with a single join, up to 23 inches diameter. The measurement of the internal stresses shown in polarised light was made possible by superposing at any given point a test piece of celluloid, which was compressed by known forces until neutralisation was obtained, as shown by the appearance of a black patch. In this way it was possible to see visually the lines of stress in the tension and compression of models of beams, girders, and struts. Other models enabled the observer to watch the increase of stress during the coiling of a spring. The effect of notches in the side of a beam, and of rivet-holes in a girder in augmenting the stresses and increasing the liability to fracture, was also exhibited.

# SOME MODERN TELESCOPE CONSTRUCTIONS.

### BY MR. ARTHUR R. HINKS, M.A.

(Chief Assistant at Cambridge Observatory).

*Lecture delivered on Thursday, January 11th, 1912.*

MR. ARTHUR R. HINKS, M.A. (Chief Assistant at the Cambridge Observatory), first described the 19-ft. *coudé* equatorial erected at Cambridge to the design of Sir Howard Grubb. The bearings of the polar tube were mounted on heavy concrete piers, the upper end of the tube entering the observer's room, while at its lower end a second tube was pivoted at right angles and carried the object glass, the rays from which were reflected to the eye-piece by a plane mirror. This mirror was caused to bisect the angle formed between the tubes by means of flexible steel bands passing round drums, one of half the diameter of the other. The driving mechanism was the only example of Grubb's electrically controlled clockwork thus far installed in this country. It was timed from a 120 lb. free pendulum bob, which made electrical contact through a blob of mercury at the lowest point of each excursion. The pendulum was given an impulse every now and then, as might be necessary. An exactly uniform movement was not wanted for the telescope, owing to the influence of refraction, which was responsible for a losing rate of from two to three hundred seconds daily. In order to compensate the effect of refraction the observer had to be able to vary the rate of the pendulum, which was done by means of lumps of lead placed in or removed from a dish fixed half-way up the pendulum, each ounce of the lead corresponding to about 10 seconds per day. The objective was a Cooke photo-visual of 12½ins.; and it had the great advantage that the visual qualities were as good as the photographic. Unfortunately, they could not get so large a glass as they would have liked. A difficulty they found was that mottled patches and threads appeared on the surface of the lens, as had been described by Sir Norman Lockyer; but a much more serious defect was that the objective was highly subject to temperature changes.

Mr. Hinks then referred to an important paper on "The Modern Reflecting Telescope," contributed to the proceedings of the Smithsonian Institution by Prof. G. W. Ritchie, of the Yerkes Observatory. The methods used by that gentleman in constructing his reflecting surfaces were of a very elaborate nature, and correspondingly expensive; but having the advantage of large funds at his disposal, Mr. Ritchie had produced extraordinarily perfect instruments. He had made spherical mirrors of 20 feet focal length, with zonal errors not amounting to one-hundredth of an inch, as shown by the application of the knife-edge test at the centre of curvature. For the testing of a paraboloid, after the spherical mirror had been figured to that curvature, Prof. Ritchie used a plane mirror of the same diameter; and that flat surface was in the first place tested by means of the spherical mirror (by successive reflection of the rays from an illuminated pinhole at the centre of curvature). Then this paraboloidal mirror was used for testing the convex hyperboloid of the telescope; and the final result was that a superfluous plane mirror was left on hand. Prof. Ritchie's methods were unnecessarily refined; as in practice it was found that the size of the photographed star images did not correspond with the indications of theory. Many of his stellar photographs, however (shown on the screen), were remarkably fine, and, in fact, unique; and his 5 ft. reflector was probably the finest piece of optical work that had ever been done and testified to the superb skill of the maker.

A noteworthy fact observed was that slight tarnishing of the silvered surface of a mirror had been found to interfere very little with its photographic efficiency. The lecturer also added the remark that they (at Cambridge) sent their objective to the makers at York for the purpose of having the alkaline efflorescence removed by dipping the affected lens in dilute sulphuric acid. That part of the work could be done in five minutes; but they did not attempt to deal with it at Cambridge because of the difficulty of correctly re-centering the lenses after taking them apart.

## A Modern "Lost Art."

The Chairman having referred to the beauty of the stellar photographs shown, and the great interest of the subject which had been so ably brought before them, invited a discussion.

Mr. Butler suggested that part of the temperature change that had been complained of in the Cambridge objective might be due not to the glass but to the air space. He mentioned, among other matters, a remarkable two lens objective at South Kensington, of which the achromatism was practically perfect. He was anxious that an investigation should be made to determine the chemical constitution of the glass, if it were possible to do so.

In reply to the Chairman, Mr. Butler said that the glass in question was manufactured by Messrs. Henri. of Paris, in 1891.

## SOME NEW VISUAL PHENOMENA.

### By Dr. F. W. Edridge-Green.

*Lecture delivered on February 8th, 1912.*

It is now nearly three years since I gave you a lecture here on the "Theory of Vision and Colour Perception." I propose to tell you a number of new facts which have been discovered by myself and others during that time, and also to show that that theory, as it was given then, has been very much strengthened. In fact, with every one of the objections that could then have been made, the statements of fact have now been found to be false, and at the present time I am not aware of, nor have I had pointed out to me, a single objection which would show that my theory of vision and colour perception is not the correct one.

Put into a few words, to go over it again, in the retina there is a space filled with fluid between the pigment cells and the rod and cone layer. In that layer there are two elements, one called rods and the other cones. In the rods there is a photo-chemical substance which is sensitive to light; in fact, a substance by which photographs have actually been taken with rabbits' and frogs' eyes of external objects. That was done many years ago. The theory that I gave you three years ago was that the elements which are called rods were not concerned as perceptive elements at all, but they let loose the visual purple into the fluid.

If we have a fluid photo-chemically sensitive, then directly light falls upon it the decomposition products will stimulate the ends of the cones, and a visual impulse be carried along the nerve fibre up to the brain. That is the theory of vision in a few words.

The centre of the retina only contains cones, and that was why the theory that I am giving you had not occurred to anyone previously, because they regarded both rods and cones as percipient elements, and therefore they could not understand how, as there was no visual purple in the centre of the retina —but simply a mass of very thin cones— the visual purple could be essential to vision. That is the point where you see best, the fovea or centre of the yellow spot of the eye. You will see that according to my theory it is absolutely essential that there should be no surplus of visual purple in the centre of the retina, because it would blur the image. If we consider this photochemical substance as sensitising the whole of the liquid stimulating the ends of the cones, there should be a mechanism regulating the flow. When a strong light falls on the eye processes are thrown out from the pigment cells, and they block the flow inwards. If we want to take a photograph, if we have a great deal of light we can do with a very slow plate. In the last few years, as I have said, all the main objections have fallen away as to the fact of this visual purple being the visual substance. Fact after fact is discovered showing its extraordinary association with vision.

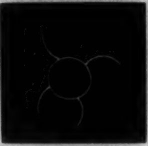
The first fact that I may mention which was published only a month or so ago, is that this visual purple, supposed to be only regenerated in the dark, is actually regenerated more quickly in the light than in the dark. That is an important point, because in all books on physiology it is stated that the visual purple is only formed in the dark, and that was the old idea, but it is now found to be disproved by making comparative experiments. Two eyes were subjected to the same bleaching process, and one was placed in the dark, and the other in moderate light. It was found, on examining the retina of the eye left exposed to light that it was purple, while the other one was quite bleached.

That is a very important and new fact. Another fact of great importance is that the curve of the threshold of stimulation by light nearly corresponds with the absorption curve of the visual purple.

The visual purple and the rods and cones have been found in all classes of vertebrates. There are nocturnal and diurnal animals, and, as would naturally be expected, animals that had to see in very feeble light would require more rods, that is to say, would require more sensitive retinas than an animal or bird that had to see by bright sunlight. It would not do to have a very considerable amount of photo-chemical substance in an eye which was subjected to the sun. There are also some invertebrates in which this visual purple has been found. Latterly it has also been found that a butterfly has a photo-chemical substance which is very rapidly decomposed and formed.

When looking at a monkey's eye, last year, I noticed that there was a small dip and four canals at the fovea. We have very much longer cones in the fovea than in other parts of the retina. The cones in the centre, where we see best, have a surface which is much greater for photo-chemical stimulation. Why should we have these cones about eight times the ordinary length if they are only sensitive to light itself and not indirectly photo-chemically? That would be a great advantage for photo-chemical stimulation, but it would be no advantage whatever for actually focusing light upon a nerve fibre. It would be obviously better if the older view of vision were correct, to have as small a point as possible. You would not have one of your best optical instruments with its focus of this length.

You will see that there is a dip for the visual purple to flow into, and, as I mentioned before, I have seen this visual purple between the cones in a monkey's retina. In a monkey's retina I noticed that not only was this little pit there but there were four little canals leading into the bigger pit. The appearance is rather a curious one, and forms a kind of star. The first thing that occurred to me when I saw those canals in the retina was that if the canals really existed in the human eye, I should be able to see them entoptically; so I devised an experiment which was successful. I could see entoptic figures due to these canals with the greatest ease, and so could other people. In fact, any good observer will not have any difficulty in seeing these canals. So that we have not only the pit, the external fovea, as it is called, but we have canals which carry this visual purple into it, when it is wanted, to enable the most sensitive part of the eye to see.



If these canals actually exist in the eye, when there was a blockage by tumour of the outflow from the eye we should get evidence of them. In one of Sir Victor Horsley's papers, the macular star can be seen. I have pictured these canals in my book,* and also in a paper in the 'Journal of Physiology,' if you care to see them.

There is one way in which you can see these canals yourself, because all here will have the apparatus, and it is a very easy experiment to perform. If you take a spectacle frame, and put in a blank disc on one side, and then look forward at a dimly lighted white wall, in a short time you will see these canals appear as a sort of star figure on the dull wall. The centre is like a whirlpool. The appearance exactly corresponds to the structure of the retina. The cones are placed so that fluid can pass easily between them.

Another way of seeing the canals entoptically is with yellow-green glass against the sky; another way in which you can see them is if you shut one eye and look at a moonlit blind. You will very soon see there is a rapid snakelike movement or whirling in the centre, streams which are continually changing their paths and running towards the centre. It gives you the impression at first of a top spinning. Another, but more unpleasant, way is to press your eye and you will then see a star figure. Helmholtz mentioned that he saw whirling currents when he pressed on his eye. You get this exact figure and all the whirling movements.

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*Hunterian Lectures on Colour Vision and Colour Blindness (Kegan, Paul & Co.).

## Yellow as a Simple Sensation.

We will now pass on to some facts directly bearing upon colour vision, facts which are crucially opposed to the old theory, the "Young-Helmholtz" theory of colour-vision—they are quite impossible to reconcile with it in any shape or sense. As you are aware, on the "Young" theory, yellow is supposed to be a mixture of red and green sensations. Yellow is a simple and not a compound colour sensation. The definite evidence of this is that if you fatigue the eye with yellow light it does not fatigue the eye in the least for either red or green.

It is quite impossible to fatigue the eye for a compound and not be able to fatigue the eye for its constituents. Therefore it is quite impossible that yellow can be a compound colour. You can make a yellow by mixing an orange-red and a yellow-green; and I am going to show with lantern slides how this may be done. But, because the orange-red and yellow-green, when mixed do make a yellow it is totally wrong to assume that our sensations are similarly constituted without direct evidence as you would have, for instance, if yellow did fatigue the eye for either red or green. Then there would be some definite evidence that yellow was a compound. You can fatigue your eyes for half an hour with yellow light, and then look at the faintest red and find it unchanged, whereas you have only got to look at red light for a second and you will not be able to see the same red five minutes afterwards.

The next fact, which I brought before the Royal Society last week, was that if you fatigue the eye for green, yellow still appears as yellow. You can easily see that if yellow consisted of red and green, if we cut out the green red should be left. Now do what you like you cannot make yellow light appear red provided you take good care to avoid all objective red light.

I have here some spectacles glazed with blue-green glass which is opaque to red light but lets through a little orange, a good deal of yellow, and most of the rest of the spectrum. If you are looking through these glasses you are obviously tiring the eye for green. As a matter of fact, an ordinary piece of white paper will very soon appear as white again, but if you look after you have tired the eye for green at spectral yellow it still appears yellow as before. It does not vary in the slightest degree, whereas you have tired the eye for green so much that white paper appears pink.

When I look through these glasses at spectrum yellow I can actually mark out the yellow band with the glasses on, taking its wave-length on either side with my spectrometer. When I take off my glasses and look at it I find that I have marked out the pure yellow, whereas one ought, if the older view were true, to have gone right up in the green, but instead of that it is neither more green nor more red; it is exactly the same wave-length as you would have marked out had you had a pure spectrum and not had the glasses on. You will be able to try this afterwards. I have prepared a series of contrasts, and you can see how absolutely the yellow remains. The result is exactly the same with these colours as with spectral colours.

Some, like Sir William Ramsay, are only able to see three colours in the spectrum. These trichromics, as I call them, when yellow is put against green, call it red, and when yellow is put against red they call it green, because they have no yellow. They do not see the yellow as red through these glasses; if anything, it inclines to green. The exaggerated contrast is abolished when we cut off objective red light. The eye in these cases becomes very rapidly fatigued for green, and when red is present it is seen, but when there is no red the eye can not be stimulated by it, but is still stimulated by the green light. I am showing you to-night a series of slides which were taken for me by Dr. F. W. Mott and his able assistant at Claybury. They are photographs of the spectrum taken by the Lumière process.

[A series of slides were then thrown on the screen and described in detail by the lecturer, who then continued as follows.]

I will conclude by giving one more example of the effect of physiological processes. If you are reading your newspaper in the early morning, the black type appears very definitely black and the white paper appears very definitely white. If you read the paper at lunch time the white paper still appears white and the print black. You will say there is no difference, but there is a difference, and an enormous one, between their appearance in the morning and at mid-day, as the intensity of light is something like 50 times as much. As white paper reflects about 15 times as much light as the print, you actually see at mid-day the print of the paper 3½ times whiter than the white paper in the morning, so that you see there are definite physiological facts which have to be taken into consideration.

It is exactly the same with colour. Lately many have been interested in the change of colour by artificial light, but the change, as we see it, is small compared with the reality. If you look through this book the colours will certainly be different by daylight and artificial light, but judging by your eye, you have no conception of the real colour.

If you take ordinary ultramarine blue paper and you look at it by gaslight, it appears an ultramarine blue, but not such a good blue as in daylight. Now take a photometer and put that ultramarine blue paper on the one side and a piece of brown paper on the other and light the brown paper by daylight and the ultramarine paper by artificial light, and the two appear identical. So that the colour you are looking at is brown. Why do we see it blue when we know that it is really by that light exactly the same colour as the brown paper? It is because we take a different relative standard of difference.

This also explains the experiment of coloured shadows. If you have a candle and a jug on the table in the morning and draw down the blinds except from one window you will notice that the candle and the daylight will each throw a shadow of the jug. The shadow of the jug which is thrown by the daylight will appear yellow, while the shadow which is thrown by the candle appears blue. The blue coloured shadow has been considered to be a subjective blue, and that was the point of view of the older physicists. I showed by varying the experiment how you could definitely show that it is not a subjective blue but that it is a relatively objective blue; that is to say, the blue shadow is objectively blue, relatively to the daylight and candlelight mixed. It cannot be that one shadow influences the other, because you can actually have a black shadow between the two.

In conclusion, to any of you who would like to see them I should be very pleased to show these contrasts.

## HOW DO WE RECOGNISE COLOUR?

*Discussion of* Dr. Edridge-Green's *Paper on Colour Vision.*

**Dr. Walmsley**, in expressing his appreciation of the interesting way in which Dr. Edridge-Green had expounded his theories, spoke of the very rapid development of this subject in recent years. So much progress had Dr. Edridge-Green and his fellow-workers made that it was impossible for men like himself with other duties, to fully appreciate these latest developments. He had fully appreciated the view of a photo chemical substance acting on the cones; and had understood Dr. Edridge-Green to say that the function of the rods was to regenerate this photo visual substance. He (Dr. Walmsley) had observed the star-like figure referred to, but had not appreciated its bearing on the theory of vision. One question he would like to raise was the bearing of this theory on the question of sharpness of focus. There seemed to be a considerable depth within which the beam of light might focus, and so affect the photo-chemical substance in different positions.

**Dr. Edridge-Green**: You can focus anywhere in this layer; but the depth is very small indeed.

**Dr. Walmsley**: Would not this affect the quality of definition?

**Mr. Chalmers**: The possible depth of focus is of the same order as that due to diffraction.

**Mr. Hughes** said he had been much interested in Dr. Edridge-Green's lecture, because from time to time he handled Holmgren's wools, in regard to which there was a great deal of confusion in separating the colours; and it seemed to him that Dr. Edridge-Green's theory accounted for this difficulty. He should like to know Dr. Edridge-Green's opinion with regard to the action of the fluid on the cones.

**Dr. Edridge-Green** remarked that light and colour were two absolutely distinct things, and there was light perception and colour perception. If decomposition of the photo-chemical substance gave rise to an electrical stimulus, that would do for the theory of light; but the point was: how did we get colour?

A cone magnified still more was like a pyramid made up of about 18 different discs. The correspondence between the number of colours they could see and the number of discs in the cone was rather remarkable. When the fluid was decomposed, perhaps, one of those discs took up certain vibrations. They had discs of different sizes in the ends of those cones, and those might be able, when the molecules or atoms took up the vibrations of light, to convey them. When they had the impulse and an additional quality to the impulse, all they wanted for colour was to have a sort of irregularity of vibration of the nerve cells which they would recognise as colour.

**Mr. Chalmers**, in expressing his thanks to Dr. Edridge-Green, said: I would like to make one or two suggestions. I would suggest to Dr. Edridge-Green that he accept in a way some of the ideas of the older physicists and use them in conjunction with his own. That as regards colour alone this might be due to a physical vibration set up in the cone, while the sensation of light is due to the visual purple, and its photo-chemical properties. The mechanism which Dr. Edridge-Green has so carefully explained seems to be the mechanism of light perception rather than colour perception, but as the light wave comes up it must set the cone in vibration, and resonance effects are likely to arise. The descriptions which Dr. Edridge-Green has given of the formation of the cone seem to bear out this hypothesis; the individual portions might well resonate to the individual colours. Any experimental evidence which tended to show that fatigue for colour was not necessarily associated with fatigue for light would support this hypothesis. It would be interesting to know how far the experiments of Professor Burch showed a possibility of fatiguing the light sense without affecting the possibility of appreciating easily the colours different from those used in fatiguing the eye. I have tried fatiguing with special colours on the lines Dr. Green suggests; but think it would be much more interesting to test the effect of fatiguing with spectrum yellow, and then examining a good yellow made by mixing a pure red and a pure green. With the special screens now available it should not be difficult to fatigue the eye for almost any colour one chose to use, and the spectrum band used might be very narrow. As regards the spectrum photographs I think Dr. Edridge-Green has treated the Autochrome plate rather unfairly, by using a source with very little blue in it. I have obtained good blue in the spectrum, but the yellow was invariably shown as red. The enlargements of the photos seem to show that the effects of halation were present, and the photographic effect had spread to the neighbouring grains. It is possible, too, that the spectrum was not pure, and the plate affected by other light. It is remarkable how difficult it is to appreciate the purity or impurity of a colour unless one has a standard of comparison.

Mr. T. Smith: I may say that I have been intensely interested in what Dr. Green has said. There are one or two questions which I should like to ask him. First of all, he suggests that there is a separate sensation for yellow which is not either red or green. I should like to ask him whether the effect he describes is observed with pigments which appear to the eye as yellow while actually being mixtures of red and green. Does he find in such cases that after fatiguing his eye to the green light he sees a difference between the yellow and the red? I would like also to say that it seems to me there is another possible way of accounting for the experimental results on absence of fatigue which Dr. Edridge Green has described. It is—it sounds rather like an Irishism—by supposing that we distinguish colour by what we do not see rather than by what we do see. A thing is yellow—you notice the blue is absent—and what you call the yellow sensation is the name we give for the absence of blue. In the same way, when you look at green, you call it green not because there is green light coming to you, but because the red light is absent and the blue light is absent; and similarly with other colours. On such a theory as this one might expect to find the fatigue phenomena Dr. Edridge Green has described. In the first place, you have green light falling on your eye; that is to say, you notice that you have not got red or blue. Having fatigued your eye to this light, *i.e.*, having got tired of being without red and blue, you look at the yellow and your eye is at once refreshed by having red present. If you have pure red present as well as the yellow, on this theory there is no reason why you should confuse them. The suggestion may perhaps seem to Dr. Edridge Green rather fantastic, but I nevertheless venture to make it.

**Dr. Edridge-Green**: I have listened with very great interest to Mr. Chalmers' remarks. The suggestion he made is not the idea of the older physicists, but his own.

**Mr. Chalmers**: My idea was to combine your own with the older physicists'.

**Dr. Edridge-Green**: The suggestion Mr. Chalmers has made meets many of the objections I have to the theories of the older physicists. Light perception and colour perception are two distinct things, but whether the process is chemical or what is the exact method we have not got a sufficient number of facts to decide. I am working now with Professor Porter on the question of fatigue. I have not found anyone who has tried the experiment who has found the least change in the red, because of fatigue by yellow. Prof. Burch did find a change, as you know, but I think he overdid the fatiguing process. He fatigued his eye with sunlight. I think that change was due to luminosity contrast. Undoubtedly, we want to have a line spectrum, or as narrow a band as possible. The special point, in my theory, is that colour produces a different change in the impulse, that is to say, the physiological basis of the sensation of colour is a variation in the impulse, and that colour blindness is due to a defective power of discriminating these differences. In the evolution of the colour sense, red and violet first appeared, then green, then yellow. One point that Mr. Smith mentioned, I think I mentioned myself, that is that nearly all colours are due to the removal of certain rays which gives a very intense colour to the remainder. The removal of the yellow or blue from the field gives a very intense complementary colour.

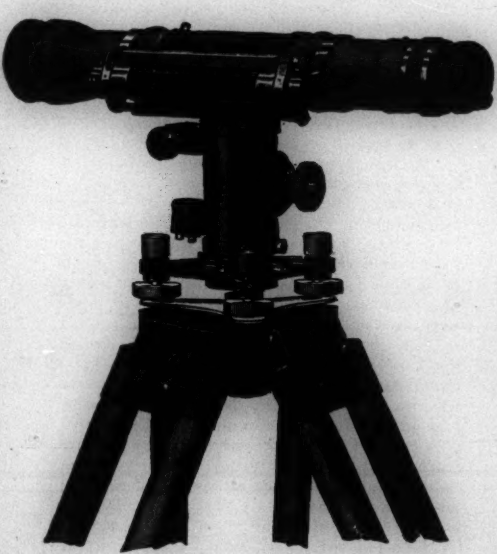
## SOME NEW OPTICAL INSTRUMENTS.

DEMONSTRATIONS BY MR. MAX POSER.

*Thursday March 14th, 1912.*

### The Improved Zeiss Level.

Mr. Poser removed the level—which weighs only 46 ozs.—from a case measuring about 8 × 5½ × 2½ ins., and mounted it on the tripod in position for use, explaining the various adjustments. The instrument is set by bringing two images of the ends of the bubble (reflected by a prism, and viewed from the eyepiece-end in a window) into optical coincidence, which is effected by means of an inclination screw. The telescope tube is revolvable on its optic axis, so that the level can be employed equally well on either side of the telescope. The tripod stand is strongly made in wood and metal to withstand climatic changes; the legs are stiffened with bolted metal plates, and provided with spurs so that the pointed ends can be forced into hard ground by pressure from the foot. The vertical axis upon which the level turns is cylindrical instead of conical; and thus, not having to carry any weight, the bearing surface wears much better. The matter has been put beyond doubt by an experimental investigation. It was found desirable to finish this cylindrical surface with the turning tool, as after grinding, even with such a material as water of Ayrstone, particles of the abrasive would be found embedded in the metal, and consequently would always act as a grinding agent. Such particles can be traced easily with the microscope, and it is impossible to get rid of them. This shows that an axis prepared by the turning method alone will easily prove more durable than one ground in. The double lever action for tilting the level enables a relatively coarse micrometric screw to be used; and on this account the life of the screw is much prolonged. The three levelling screws terminate in ball and socket arrangements (the male terminals on the screws) whereby the general levelling with a small circular level is done without causing any unequal strain on the screw. The eyepiece and objective are fixtures,



THE NEWEST MODEL SURVEYOR'S LEVEL.

and the telescope is focused by adjusting an intermediate lens, this purpose being served by an external milled head; thus the tube can be hermetically sealed, and dust and moisture perfectly excluded. The reversing level by itself is not a new feature, but what is quite new is the device by which the optical system is made reversible. The reversibility of both telescope and level enables four independent readings to be made, of which the mean is free from instrumental error; and to that mean the entire instrument must be adjusted. The whole process of putting the instrument into adjustment need not occupy more than five minutes. When using the level for practical work, of course it is only necessary to read the staff when the level is in the first, and perhaps in the second position for most precise readings. The stadia lines represent 1-100 as usual, and the ratio is not affected for near distances, since focusing is done with the intermediate lens—eyepiece and objective occupying fixed positions.

The Chairman, at the conclusion of Mr. Poser's demonstration, said they had been all very much interested in the new instrument, and particularly in the employment of the auxiliary focusing lens, which was a valuable feature. He believed, however, that the device had been used before.

Mr. Poser remarked that the supplementary lens had not been applied before in conjunction with a reversible telescope. With this Mr. Chalmers agreed.

Mr. A. Hughes said he would just express the great interest he felt in the many new features of the Zeiss level, particularly those contributing to the portability and greater accuracy of the instrument.

Mr. Smith asked to what extent error would be introduced if the mechanical axis about which the telescope turned were not exactly on the optical axis. Also what were the optical data of the lens system used? What was the intermediate lens?

The lecturer replied that the instrument could be set within two seconds of arc (one division of the micrometer screw head). The accuracy of the level itself was within ten seconds. The magnification was 30 diameters with the larger level and 20 with the small. The intermediate lens was a positive one, varying the focus from five yards to infinity. The objective was a doublet. The eyepiece, an aplanat, was focusable in terms of diopters, to adapt it for the eyes of different observers.

Mr. H. A. Hughes, with reference to the cylindrical axis, asked about wear by the weight of the instrument upon the surface.

The lecturer said that the cylindrical axis merely formed a lateral guide. The sleeve surrounding it was free at the lower end, and the whole weight was carried by a point which rested on top of the cylinder.

Mr. H. A. Hughes remarked that he could not approve the principle of resting the weight on a point, in view of the effects of wind pressure or vibrations. He did not see the advantage gained over the ordinary cone, which was usually cleared away in the centre, thereby making a sufficient allowance for dust. In course of time a level with a cylindrical axis would become difficult to renovate, as the polished cylinder might have to be renewed. He would like to know also how condensation was prevented on the auxiliary lens, even although it was sealed in as well as possible.

The lecturer said he considered that what was material with regard to the axis was the question of fit. Their cylindrical axis gave them a better guide, and therefore greater stability than the ordinary cone. The recess made in that cone was a source of weakness, although of course it was much easier to fit the cone, at top and bottom only. As for the condensation of moisture on the inner lens surfaces, this could only happen if the air had access to some extent to the interior; and that was prevented by sealing up the instrument.

Mr. H. A. Hughes pointed out that there must be a certain amount of air within the level in the first place. And under given meteorological conditions he was certain there would be condensation. How then could the lenses be got at?

The lecturer replied that the level could be taken to pieces in a few minutes; but it was not necessary. The conditions were just the same in their prismatic field glasses, which were sealed up in a similar way in order to avoid that very trouble, and prevent dust, &c., gaining access to the interior. Although many thousands of the prismatic glasses were in use, it had not been found that condensation occurred. Also the sealing up answered perfectly to prevent the development of a fungus that was apt to appear inside such instruments in tropical climates.

In reply to the Chairman, Mr. Poser added that no special precautions were taken for drying the warm air in the adjusting rooms. These were, however, well oiled in order to prevent as far as possible the presence of dust in the air.

Mr. Gilbert asked about the effect of rough usage on the bearing point over the cylinder.

The lecturer said there was practically no wear whatever on that bearing point; but supposing there should be any, it would not have the least effect on the action of the cylindrical axis.

Mr. Gilbert pointed out that should the level have a fall and the axis become bent, an ordinary repairer, far abroad, would have more difficulty with the cylinder than with the usual cone.

The lecturer explained that the axis was very strong and the gun-metal sleeve used was not made of tubing but drilled out of the solid; so it was not likely to come to harm, unless the instrument received such a blow that the entire optical system would be smashed at the same time. For the sake of both accuracy and cheapness the cylinders were turned by automatic machinery. An ordinary repairer might find it no easy matter to duplicate the cylinder by hand work; but as a temporary expedient he could turn bearing surfaces at the two ends of a cylinder recessed in the middle—which would serve until an opportunity was found for having the level put into proper condition again.

## Demonstration of New Compass and Sextant.

### By Mr. H. A. Hughes.

Mr. H. A. Hughes was next called on by the Chairman to demonstrate two novel instruments. One was a magnetic compass having the outer zone of the card transparent excepting for an index intended, of course, to be set to point to the lubber's line—the instrument having glass plates above and below, so as to adapt it for aeronaut's use, the course being observed by watching the line of apparent motion of objects below. The card was also advantageously adapted to luminous compasses for night marching. The other instrument was a sextant having a fine tangent screw embracing a reading in either direction of 2°, with a spring clamping device whereby, on reaching this limit, the screw could be instantly returned to the middle of its run so as to extend the reading.

The Chairman said that the new sextant was a most interesting development.

Mr. Poser remarked that in using the aeronaut's compass it would be found that at a great height above the earth the apparent motion of objects was small, and would give an unsatisfactory alignment.

Mr. Hughes replied that the compass had been found satisfactory by aeroplanists at the customary heights.

## Spherical Aberration Effects.

BY MR. S. D. CHALMERS.

Mr. Chalmers then said that he proposed to introduce one or two matters which might be of some interest to the members; and he hoped by thus setting the example in a direction he had indicated at a previous meeting, to encourage others to bring forward from time to time anything they thought likely to interest upon the ground of novelty or otherwise. He gave first a demonstration of retinoscopy, in which the optical system of the projection lantern took the place of that of the eye, and the "shadow" was shown traversing the screen in different directions corresponding to variations of the focal power. Secondly, a number of experiments were shown illustrating the effects of diaphragm aperture, and, in some instances, of diffraction, on the resolution or definition of lines in the projected image of a clock-face astigmatic chart.

# COLOUR PHOTOGRAPHY FOR OPTICIANS.

## The Portrayal of Fundus Details for Purposes of Instruction.

By Messrs. H. A. Boatman and H. C. Patey.

*Paper read April 11th, 1912.*

The object of the demonstration we are bringing before you this evening is to suggest a means whereby the characteristic appearances of the fundus, in the original the work of expert ophthalmoscopic artists, can be most suitably reproduced for the purposes of demonstration and instruction.

It forms no part of our purpose to discuss whether the ability to recognise certain pathological conditions of the fundus should form part of the training of the qualified ophthalmic optician—there is much to be said for and against such training—but, whatever view be taken of this matter, it is evident that methods for disseminating and duplicating the best original drawings of the fundus must be useful to students of this subject.

The details to be recorded in the case of the fundus are very largely those of colour as well as of form, hence the special utility of colour photography It is, of course, possible to directly photograph the fundus, as has been done, for instance, by Prof. Dimmer, of Grätz.

But there are several reasons why direct methods of photography, whether in colour or otherwise, do not lend themselves to the best portrayal of the fundus details. The difficulties that arise are not only those of illumination and exposure, but are inherent in the nature of the detail to be recorded.

A skilled ophthalmoscopist, such, for instance, as Mr. Head, who is responsible for the beautiful drawings in the various works which have been published by Dr. Lindsay Johnson, will always see more detail than is possible to the photographic plate. The case is somewhat analagous to the uses of photography in astronomical work. A faint image not appreciable by the eye may be recorded by the *additive* effect of a long exposure, but for the recording of fine detail, dependent on taking advantage of moments of good seeing, as, for instance, in the recording of planetary markings, the human perception is of vastly greater accuracy.

It is rather as an adjunct, therefore, to the work of a trained ophthalmoscopic artist that photography in general, and colour photography in particular, will be of assistance in disseminating a knowledge of the fundus.

The slides shown to-night are, in the main, reproductions from the lithographs in Haab's "Atlas of Ophthalmoscopy," and the results would doubtless be better still if original drawings had been available, for the autochrome process is, as regards, at any rate, the faithful reproduction of colour detail, superior to lithography. This process, by which the slides have been produced, is in itself a very pretty demonstration of the theory of colour vision known as the "tri-colour theory."

According to the theory first put forward by Thomas Young and elaborated by Helmholtz, it is supposed that the eye perceives colours by the stimulation of three primary colour sense-organs affected respectively by red, green, and blue violet light, the joint action of the three stimulations producing the sensation of white light.

The methods of tri-colour photography employed to reproduce colour have nearly all been based on this theory, but up to the time of the autochrome plate the processes have involved, generally speaking, two separate and distinct operations. The first process obtained a *record* of colour, the second produced the colours of the original from this record, but the autochrome plate has united and simplified these two operations, and shows that from a simple basis of three primary elements all lines and gradations of colour can be represented.

It indicates, perhaps, that no great complication is necessitated in the sensory apparatus of the retina, whatever this may be, which would suffice to give rise to different nerve sensations for the different wave lengths of white light, or, in other words, to produce colour vision.

The autochrome plate consists of an ordinary sensitive panchromatic emulsion not coated directly on the glass support, as in an ordinary plate, but separated therefrom by a layer known as the starch grain layer, in which reside the colour producing properties. This starch grain layer consists of nearly equal proportions of red, green, and blue violet starch grains lying (one-ply) on the glass support, any interstices which may occur between them being filled in the process of manufacture with an opaque black pigment. The starch grains are separated from the sensitive emulsion by a layer of hard varnish, which protects them from any action of the solutions, etc., which are used for developing the plate.

The plate is exposed *glass side* towards the lens which forms the image, and a special yellow screen is used to correct for the greater sensitiveness of the emulsion to blue light.

With the screen in position the autochrome plate is *equally* sensitive to red, green, or blue light. It is now not difficult to trace the action which takes place when a coloured image falls on the starch grain layer.

Suppose this happens to be red light. Then all such light falling on the green and blue starch grains will be absorbed, and none will pass through them to affect the sensitive emulsion behind. Behind the red starch grains, however, the silver emulsion will be acted upon, and when the autochrome plate is developed a black spot will appear behind each red starch grain.

Such development forms the first of the three chief operations to be performed when producing a coloured photograph. If at this stage the plate be fixed and held to the light it will not appear red, as was the colour of the object photographed, but blue-green, the complementary colour of the red; for behind each red starch grain is a black dot which stops all light passing.

The next, our second step, therefore, is to dissolve away the reduced silver behind the red starch grains, and this is done in a solution of potassium permanganate and sulphuric acid. At the end of this operation the original red of the object photographed will begin to appear.

A certain amount of light will still, however, get through the *unreduced* silver behind the blue and green starch grains; and the third and last operation, re-development, completes the process. This is done by exposing the plate to light and developing with an Amidol or Rodinal developer, which blackens the silver behind the blue and green starch grains, and only allows it to pass through the red starch grains, and the object now appears in its pure original colours.

The same action takes place in respect of any other colours, intermediate colours causing partial actions on *two* or *three* of the three primary starch grain elements. Yellow, for instance, is produced by an equal action of the red and green starch grains.

If necessary, the autochrome plate is intensified in order to increase the brilliancy of the colours; and varnishing to protect the very delicate film completes the process.

[At the conclusion of the lecture a remarkably fine series of autochrome slides were projected on the lantern screen, illustrating normal and pathological conditions of the fundus; and the significance of the various pictures shown was fully explained in each case by Dr. Ettles.]

# ON THE PRODUCTION OF LIGHT BY LIVING BEINGS.

By Dr. Max Coque.

*November 14th, 1912.*

I FELT deeply honoured when I was asked to address you this evening, and it was with very sincere pleasure that I devoted some of my scanty spare time to the preparation of this lecture. Lecture, I say, but really "notes" would be a better way of putting it, for when I came to look at this subject, upon which, you must understand, I lay no claim to being an authority, I found I could only outline the main facts and suggest a few ideas which, I trust, will rouse some of my hearers to pursue the study of this interesting subject still further. Although I quite agree with Prof. Silvanus Thompson when he says that "all who are really interested in any scientific subject, are interested not only in the subject itself, as it stands at the present day and in its future advances, but they are also interested in knowing how that science has come to be what it is, for no sciences have come into being full-fledged," yet I have had to cut out the history of the various theories of optics as opening up matter of too vast a nature to be adequately dealt with in a short address.

Before going into the subject of production of light by living beings, I think a few words on the nature of light, on the chief methods of manufacturing light, may prove useful.

Everybody is acquainted with Newton's great discovery that white light really consists of a mixture of lights of different colours—red, orange, yellow, green, blue and violet. All ordinary sources of light, natural or artificial, send out these mixtures of colours. To sort them out we have only to look at the light through a glass prism, or to let a beam of light pass through such a prism. The prisms refract the lights of different colours through different angles, according to their wave-lengths, and give us the rainbow coloured patch we call the spectrum.

As it is well known, light consists of innumerable waves of extraordinary minuteness. Light of each colour possesses its own particular wave-length, or, more exactly, light of each particular wave-length has its own particular colour, the colour being to light what pitch is to sound. These wave-lengths are so small that they must be measured in millionths of an inch. Thus, light having a wave-length of 27 to 30 millionths of an inch produces a red sensation in the eye, and we call it red. Waves of about 15 to 16 millionths of an inch give the sensation of violet. Those of 20 millionths of an inch, the sensation of green.

Since all incandescent bodies give out waves of all sorts and sizes, they shine with all the different colours at once and give out white light.

But all our sources of illumination are not equally white in the light they give. This is so, because a true white is not produced unless a proper proportion is preserved between the component colours. If in the mixture there is too much red, the resulting light will be a reddish white. If too much green, the light will be greenish, and so forth.

For a complete study, then, of the light of any source, we ought not only to measure its candle power but also to analyse the light to find out its composition. This is done in the same way as Newton analysed solar light, that is, the spectrum of the flame is observed.

When we look at a spectrum, we must understand that really the spectrum extends much farther than the visible limits. To the visible rays of longest wave-length is due the red colour of the extreme left.* Waves of somewhat shorter length produce the adjoining strip of orange, and the succeeding colours, yellow, green, blue, correspond respectively to waves of shorter and shorter lengths. Lastly, there comes a patch of violet due to those of the visible rays whose wave-length is the shortest of all. The wave-length of the light of the extreme red is about 1/34,000 inch, and as we pass along the spectrum the wave-length diminishes gradually until at the extreme outer edge of the violet it is about 1/64,000 inch, and not much more than half that of the other end.

The two ends of the spectrum gradually fade away into darkness, and the point upon which I wish to insist is this: The position of the boundaries terminating the visible spectrum does not depend upon anything whatever in the nature of light regarded as a physical phenomenon.

Ether waves which are much longer and much shorter than those which illuminate the spectrum, certainly exist, and evidence of their existence is easily obtainable. But we cannot see them; they fall upon our eye without exciting the faintest sensation of light. We are blind to the waves that are either shorter than the violet ones or longer than the red ones. The proofs that there are these invisible radiations are very simple. A thermometer placed beyond the red end will show that there are dark radiations, heat waves, in this region, and a bit of photographic paper placed in the region beyond the violet will be darkened, showing that at that end there are some invisible chemical rays.

The visible spectrum is limited solely by the physiological constitution of our organs of vision, and the fact that it begins and ends where it does, from a physical point of view, is a mere accident. The spectrum actually projected upon the screen is, in truth, much longer than that portion of it which any one can see; it extends for a considerable distance beyond the violet at one end and beyond the red at the other, these invisible portions being known as the ultra-violet and infra-red regions. Peoples' eyes differ in regard to range of sensibility, just as their ears do. I believe the sensibility of my own eyes to be normal, but if I were to indicate the two points where the spectrum appears to me to begin and to end, a great many persons would certainly be inclined to disagree with me and place the boundaries somewhere else. Some indeed could see nothing whatever in what appears to most of us to be a brilliant portion of the red.

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* A coloured spectrum was projected on the screen.

Again, it is by no means probable that in all animals and insects the limits of vision are the same as they are in man. To quote Shalford Bidwell: "We might naturally expect that larger, and perhaps more coarsely, constructed eyes than our own would correspond to waves of greater average length, while the visual organs of small insects might, on the other hand, be more sensitive to shorter waves. The point is not one that can be easily settled, because we are unable to cross-examine an animal as to what it sees under different conditions. But Sir John Lubbock, taking advantage of the dislike which ants when in their nest have for light, has proved by a series of very exhaustive and conclusive experiments that these insects are most sensitive to rays which our own eyes cannot perceive at all. That region of the spectrum which appears brightest to the eye of an ant is what we should call a perfectly dark one, lying outside the violet, where the incident waves have a length of less than 1/64,000 inch." We shall see presently, however, that all authorities do not agree on this point.

Let us briefly examine now the different ways of manufacturing light.

One of the most important directions in which man has subdued the forces of nature is certainly the artificial production of light, which enables him to extend the day far into the night. Not perhaps an unmixed blessing though, since one of the conditions nature has imposed upon man in order that he shall retain his health is that he must rest or sleep during a certain number of hours each day. If he fails to take this rest he will, sooner or later, pay dearly for his disregard of the laws of nature. It would be impossible nowadays to come back to the curfew bell of the good old times, on the ringing of which all lights were to be extinguished and all well-behaved citizens had to be in bed. The world has gone too far for that. The curfew is a thing of the past. Night work is now essential. It is during the small hours of the night, while most people are asleep, that the food products are brought into big towns, that the great newspapers are printed, so that they can be placed next morning on the breakfast table. In fact, should artificial light be no longer employed, the world would be sent back in its civilisation to an extent that would seem almost incredible.

Man's first invention for turning night into day was crude indeed, a mere burning stick snatched from the fire. It was a poor means of illumination, yet it permitted him to go some distance from his dwelling, and, to a certain extent, to prolong the time during which he could attend to his work. The next improvement was the discovery of the fact that animal fat and oils would burn, and that putting a bit of dry wood into a shell filled with oil, constituted a simple lamp. Such primitive lamps, made of rudely hollowed stones, of burnt clay, or of bronze, are found amongst the relics of Egyptian tombs and in the excavations of Pompeii, and may be seen in museums.

Fish oils and animal oils only were used. Although the apothecary of the middle ages knew vegetable oils pressed out from seeds, these oils were not used for illuminating purposes till the 18th century, and for a considerable period the only improvement on the primitive lamp just described was the discovery of the rush light and the candle. It had been found long ago that a piece of wood or a rope of grass dipped into melted fat makes a good torch, and the first candle consisted of a dried rush dipped into molten tallow. To replace the dried rush by a woven wick and to devise means of casting tallow, and later wax, round the wick in a mould, were improvements introduced about the end of the 18th century. Early in the nineteenth century, the invention of coal gas came as a stupendous progress in the problem of illumination; it was in 1810 that the Gas Light and Coke Co. was incorporated to light the streets of London. Very nearly at the same time, Humphry Davy showed the first electric arc lamp in the theatre of the Royal Institution. His source of energy was a battery of 150 pairs of plate. Thirty years later, the invention of the principle of the dynamo by Faraday gave a simple and efficient way of generating the electric current mechanically and rendered practical the use of the electric glow lamp.

In the case of most artificial lights, the evolution of light is the result of *incandescence*. The common process involved is that something is made very hot and shines because it is very hot. In fact, incandescence is the shining of hot bodies because they are hot. It is a well-known fact that if we take any solid thing, say a piece of iron, or a brick, and heat it enough, it will become red hot, that is, it will emit red light. If brought to a higher temperature, it shines more brightly and gives out not only more light, but light of a whiter kind. If it is less heated, it still emits radiations, but radiations of a kind we cannot see, though they can be felt as heat, by holding one's hand near the object. So in all cases of incandescence the incandescent substance sends out a lot of heat, as well as some light.

As we will see presently, one of the scientific problems of the day is to find a source of light which shall give light without heat. All our sources of light are deplorably wasteful. They burn up a lot of gas or oil, or use up an electric current and waste the greater part of it in emitting heat rays we don't want and utilise very little of it in generating the light rays we do want.

We have seen that all solid bodies when heated hot enough become incandescent. The brightness of the ordinary flames, like that of coal gas, paraffin lamp, candles, etc., is due to there being solid particles in them. If we burn these substances so that no solid particles are present in the flame, we get very little light indeed, but the flame becomes very hot. This can be shown by the atmospheric gas burner—a Bunsen burner. It gives a flame very little luminous, but very hot. Stopping the hole by which air is admitted renders the flame at once brighter, larger, but less hot. A similar effect is observed when the solid particles in a candle flame are burnt by means of a blowpipe.

Everybody has observed that the flame of an ordinary candle or gas jet is not of uniform brightness, there is always a bluish or nearly non-luminous part at the bottom below the bright part, and this blue really extends round the flame. It is here that the coal gas burns with plenty of air, as in the Bunsen burner, and the heat it gives out decomposes the gas within the flame and causes the separation of a sheet of carbon, which glows partly because it is in a very hot region, partly because it burns away like so many bits of charcoal.

Here is a gas flame given by an ordinary batswing burner. Dr. Smithells, of Leeds, the principal authority on flames, has shown that such a flame consists really of two flat burning surfaces, with two sheets of glowing particles and a cooler layer of unburned glass between. The temperature varies from point to point, and has been mapped out. In some parts, it is sufficient to melt a fine platinum wire, and in that neighbourhood the glow of the carbon particles is, of course, very bright. The temperatures marked at the side are on an arbitrary scale, lower than their true centigrade. The brightness at the upper part is due to the particles of carbon formed in the lower part.

Every flame—that of a candle for instance, is in fact a gas flame, the lower part of the flame and the wick acting as a miniature gas factory, whereas tallow or wax is distilled in place of coal, and when it was recognised that the brightness of a flame was due to radiation from solid matter, it was an obvious suggestion to try to get more light from gas by burning it with the hottest sort of flame, the non-luminous one, and then inserting in the flame a network of fine wire. Forty years ago, the town of Nantes was lit with such burners, consisting of a Bunsen burner, in which a mantle of wire gauze was hanging, but they were not successful, being too perishable.

However, the idea of incandescence was found and the problem was solved in a practical way when Dr. Auer von Welsbach introduced in 1883 the well-known Welsbach mantle, consisting of a light fabric of ramie fibres woven or knitted with an open mesh, soaked in a chemical solution of the rare earths, erbiah, yttria zirconia, thoria, etc. These substances are all white and resemble lime.

I cannot do more than mention incandescence by electricity, and I come to another totally different way of manufacturing light.

So far we have dealt with the production of light by incandescence, light being the consequence of heating, but there are examples in nature of light produced without heat.

It has been known from early times that a substance called Bologna stone possesses the curious property, after exposure to light, of continuing for many hours to emit in the dark a pale light. Bologna stone, or Bologna phosphorus, was prepared by the partial calcination of a certain mineral called heavy spar or sulphate of barium, and afterwards reducing it to a fine powder. The sulphide of calcium, or Canton's phosphorus, possesses the same property to a very high degree. In more recent times, the latter material has been used in the manufacture of what are called luminous paints. Everybody is familiar with such paints coating the surfaces of match boxes. During their exposure to the light in daytime, these paints are so affected that they continue to shine during the greater part of the night, although there is no other light in the room. Another practical application of luminous paints is its use over the surface of a clock dial. It must be observed, though, that in all these cases, the light produced in this way by phosphorescence is exceedingly feeble.

There are many substances which, like Bologna stone and luminous paints, possess, to a greater or less degree, the power of continuing to shine after exposure to light. Some of these, as in the case of the luminous paint, continue to show light for many hours after daylight ceases to fall on them, others continue to shine but for a few moments.

The following substances possess phosphorescence in varying degrees: sulphides of barium and calcium; diamond, a variety of fluorspar known as chlorophane, dry paper, sugar, salts of alkalies and alkaline earths, compounds of uranium, etc.

Phosphorescence can be produced by exposure to all portions of the spectrum; not only to the ultra-violet rays, but also to all the colours between red and violet, and even to the invisible rays in the infra-red and ultra-violet.

Whilst the above phosphorescent substances give light after they have been exposed to the light of the sun or placed in some parts of the spectrum, some other substances are made to emit a beautiful bluish light when the invisible ultra-violet rays of the spectrum are focused upon them. A solution of bisulphate of quinine shows this effect in the most marked manner. Such substances have the power of slowing down the ultra-violet radiations so as to permit them to become visible. When the bark of the common horse-chestnut tree is boiled in water, the clear solution is invisible in ordinary light, but gives out a bluish light when brought into the ultra-volet rays of the spectrum. The simplest way to obtain this light is to receive the spectrum on a sheet of paper soaked in the solution of horse-chestnut bark; the length of the spectrum is then greatly increased, for instead of being visible only between red and violet, it now extends far beyond the violet.

The light of the electric arc lamp produces a quantity of ultra-violet rays greater than that of the solar light. On the other hand, quartz is almost completely transparent to ultra-violet rays and differs in this respect from glass, which is opaque to a large percentage of such rays. If a beam of light proceeding from an electric arc lamp is caused to pass through a quartz prism, the length of the ultra-violet spectrum will be from six to eight times longer than the distance between the red and the violet ends.

If such a spectrum is received on a sheet of paper moistened with the water in which the bark of the chestnut tree has been boiled, the length of the visible spectrum will be considerably increased. A space beyond the violet, of from six to eight times the distance between the red and the violet rays, will now shine with fluorescent light.

Everybody is familiar with the greenish coloured glass, known as uranium glass, the colour of which is due to the presence of a small quantity of a salt of the metal uranium. This glass has the power of fluorescence to a high degree, and if a beam of ultra-violet rays is thrown within its mass, the path of the rays is made visible as a bright green light, for the rays, as they pass through the glass, become luminous by fluorescence.

Some of the many other substances that also possess the power of fluorescence, are the green colouring matter of leaves, obtained by soaking bruised leaves in alcohol, ordinary petroleum, a solution of turmeric alcohol, etc. Let us mention the fact that the word fluorescence has been given to this peculiar property of light because its effects were first showed in fluorspar.

If a fluorescent substance is exposed in a darkened room to light from different sources, a difference will be observed in the intensity of the fluorescent light produced. While ordinary sunlight possesses this power in a fairly marked degree, it is surpassed either by the light of the electric arc lamp, or by the light emitted by a burning magnesium ribbon. The reason is obvious; it is the ultra-violet rays that produce fluorescence, and the light emitted by the burning magnesium wire, or the electric arc, contains a greater proportion of the ultra-violet rays than does sunlight. A curious experiment can be performed to illustrate fluorescence. If we soak stramonium leaves in water, we get an almost colourless solution, and writing on a sheet of white paper by means of a pen dipped in this solution will be practically invisible, but if the light from burning sulphur or brimstone is allowed to fall upon it, it instantly becomes visible by fluorescence.

Some substances possess the power of fluorescence to a high degree. Two of the most remarkable fluorescent substances are the platinocyanide of barium and the tungstate of calcium. Such substances are employed for covering the screen in the apparatus known as fluoroscope.

Fluorescence and phosphorescence differ in a very marked degree. While fluorescence is only excited by ultra-violet rays, phosphorescence can be produced by exposure to all portions of the spectrum, not only to the ultra-violet rays, but also to all the colours between the violet and the red, and even to the rays of the infra-red portion. When the ultra-violet rays produce phosphorescence or fluorescence, their two rapid motions have to be slowed down until it can affect the eye as light, while in the case of the invisible rays beyond the red, the too slow vibrations or ether waves have to be increased or accelerated, until they have become sufficiently rapid to affect the eye. A very pretty experiment can illustrate the latter point. Let us heat a dull iron plate in a dark room by allowing the flame of a Bunsen burner to fall directly on its lower surface, being careful not to render the plate incandescent. Now, looking at the upper surface by placing our eye in such a position that the flame of the Bunsen burner is not visible, let us throw a few fragments of the variety of fluorspar known as chlorophane, on the upper surface of the plate. Immediately a brilliant emerald green phosphorescent light will be emitted by the fragments. This light has been produced by increase in the rapidity of the heat waves from the iron plate to such an extent as to enable them to affect the eye.

The term luminescence has been coined to denote the production of light by other means than incandescence. The kind of light thus produced by phosphorescence or by fluorescence is called cold light, and the category of such light has been vastly increased in recent years by the discoveries of Sir William Crookes and others, of the possibility of making substances shine in the dark by exposing them to the special kinds of radiation known as Kathoda rays. We cannot enter into this subject. Let us simply state for the present that the phosphorescence of the sea on a summer night is due to living animals that possess to a marked degree the power of producing phosphorescent light. The exquisite pale blue light produced by the glow-worm, the firefly and some other living beings, is of the same kind and will be investigated a little later.

Before coming to the subject of production of physiologic light, I think a brief review of the influence of light on living beings may be interesting.

The influence of light, that is, of that part of the spectrum to which our retina is sensitive, on living beings, has been the object of numerous researches, but it is not always easy to distinguish in every case the effects produced by the calorific or the chemical radiations from that produced by the light itself.

The influence of light on living organisms is manifested by modifications in the nutrition; by production of electric phenomena; by production of movement; by production of sensations and perceptions.

Our time is too short for me to do more than state the fact that light has a well-known effect on the micro-organisms producing fermentations. The sun destroys the activity of the yeast. The rennet, or ferment, which, when added to warm milk, causes the separation of curd consisting of casein and fat, has to be kept in the dark. Generally the beneficent effect of the solar rays as a means of disinfection is in itself a proof of the powerful effect of light in the destruction of some bacteria.

Light also produces a destructive action on many pigments, especially on the visual purple. This pigment is formed again in the dark and disappears in the light, and this effect enables us to obtain these retinal photographs or optograms, which may be fixed by means of a solution of alum. The discoloration of the visual purple is practically nil in red light; it increases in the green and reaches its maximum in the blue. To obtain these optographs the exposure must be relatively long, and the discoloration of the visual purple is regarded as but a secondary phenomenon, the purpose of which is probably the absorption of the excess of light entering the eye.

Light has not always a destructive effect on pigments; it often, on the contrary, determines the production of pigments. This is well proved by the effect of the solar light on parts of the skin which are unprotected by our clothes.

If we expose to light one of those curious blind creatures found in the deep caves of Adelsburg, near Trieste, or in the caves of Carniole and Dalmatia, the proteus, the body of which is white in colour, faintly touched with red, it develops a coloration which ultimately becomes almost black. This action may be localised to certain points of the surface of the body which are illuminated, the other parts being protected from light. The coloration is due to the formation of a pigment which becomes deposited in the most vascular parts of the skin, especially around capillary vessels. This pigment appears to be due to an extravasation of the blood. A red light does not cause the production of pigment, and more pigment is produced in green light than in blue light, although blue light is that which the animal tries to escape from, and which, we may infer, is most disagreeable to it. The pigmentation disappears rapidly in the dark. The fact is general; we know that in miners the skin, and even the hair, become discoloured in time, apart from the discoloration observed in cases of anæmia due to a parasite.

It is well known that animals living in tropical regions present the richest and most varied colours, whereas, in polar regions, white is the dominating shade. This predominance of white in arctic regions is probably not a phenomenon of mimicry, but is the result of the insufficiency of the solar radiation.

This point seems established by the fact that the dorsal portion of polar animals is more pigmented than the under portion. Moreover, the molluscs and crustaceans living in the sea at a depth varying from 100 to 200 fathoms are white. It is true that at a much greater depth, of 1,000 and more fathoms, crustaceans and zoophytes of the brightest colours are found, but we will see presently that these abysmal regions are illuminated by the animals themselves.

The proteus likes darkness best, but he does not fear red light, and, on the other hand, it has the greatest objection to blue light, probably because of the chemical rays which accompany blue radiations.

Everybody knows the peculiar effect produced by solar light, and also by the electric arc lamp, on the portions of our body exposed to their radiations. This effect, termed sunstroke, really consists in an inflammatory process which may go as far as to produce a blister similar to those observed in a burn. This erythema solare, to give it its proper name, is frequently observed in mountaineers, although it has been found impossible to explain why the light reflected from the snow and ice of the high mountains should have a more energetic effect than that of the direct light. Coating the face with a fluorescent substance, like quinine sulphate, or covering it with a red veil, will prevent the ill effect of such a sunstroke. A layer of Indian ink acts in the same way, and this proves clearly that the effect of the sun is not due to the heat radiations, and at the same time explains the usefulness of the black pigment in the skins of negroes.

The effect on the skin of the Rontgen rays and of the emanation radiated by radium is somewhat similar to those of the chemical rays of the spectrum, although they may be much more powerful.

The influence of light in the development of the eye is shown in a striking way by a marine crustacean, ethusa granulata. At the surface the animal has well-made eyes, at the end of a movable peduncle. Between 110 and 370 fathoms, the eyes are still existing, but they consist of a round mass. Between 500 and 700 fathoms, the peduncle does not show any trace of eye.

The influence of light on the mental and psychical condition of man has been the subject of much speculation, but, unfortunately, the few well proved facts we are in possession of are mixed with a considerable amount of fancy and exaggeration.

Black, violet, blue and dark green are said to be sad shades, and are often associated with mourning, while light green, red, yellow, are regarded as gay colours. It is in great part to the light that we owe the different psychical modifications we experience in a dark or a bright day, or in a starlit night, or in a pitch dark one. Everybody knows that red excites a bull or a turkey.

Dor has observed a considerable excitation, and even acute delirium, in neurasthenic subjects directed to look at a red surface, whereas a green surface, even brightly illuminated, did not give such effect. It has been found at the photographic plate factory of the brothers Lumiere, near Lyon, that when the workshops were illuminated in red, the workmen were constantly in a condition of excitement, which has given place to a much sober frame of mind since green light has been substituted. Moreover, they feel much less tired after their day's work.

The effect of light upon the muscular work has not been well investigated, and a dynamogenic classification of the colour has not been possible yet. Griesback, though, found that manual work tries blind people sooner than seeing individuals.

The influence of light on nutrition has long been known, and light baths were used by the Greeks. Herodote states that heliosis—it is the term he used to denote light baths—were useful to those who need to improve their muscular condition. In our days, baths of artificial electric light have been used in cases of diseases characterised by the low nutrition of the patient, as anæmia, chlorosis, neurasthenia, and especially in diseases of the skin. According to Finsen, the violet and the ultra-violet radiations are chiefly active, especially in parasitic affections.

It is a well-known fact that bacteria are destroyed by light. Hence the beneficial effect of the sun as a means of sanitation.

Conversely though, the chemical radiations are very harmful in certain particular cases. It has been a long standing custom in the East to surround persons suffering with smallpox with red draperies, and recent experiments have shown that red light has a favourable effect on such subjects, as well as in scarlet fever and measles.

Finally, electric light baths, in which the effect of heat is added to that of light seem to have given good results in cases of albumenuria or Bright's disease.

The movements determined by luminous excitation in animals are varied: some animals seem to be attracted by light, others try to avoid it. The name of lucifuge or nyctalophyle is given to the latter, that of hemeralophile or nyctalophobe to the former. But Paul Bert has pointed out the fact that no undue importance must be attached to this classification. According to him, all animals seek light; it is merely a question of intensity. He has shown that some lucifuge animals, for instance, black beetles, when placed in a box quite dark, except in a corner in which a few pinholes admit a feeble light, move towards that part, but, on the other hand, they try to escape a strong light. We do practically the same, since we generally avoid the full light of the sun.

It has been asserted by some physiologists that lucifuge animals avoid light, because of the chemical rays, and that for this reason they prefer red radiations to blue and violet. According to Paul Bert, however, most animals see exactly the same part of the spectrum as we do. This eminent physiologist does not agree with Lubbock's statement that ants are strongly affected by ultra-violet rays, which are quite dark for us. There may be, perhaps, an effect quite different from those of ocular vision.

In animals possessing eyes, it is clear that ocular vision plays a considerable part in phenomena of locomotion. It is a well-known fact that if one of the lanterns of that curious luminous insect of West India, the pyrophore, is blocked, while the insect progresses in a straight line at night, by its own light, it is seen describing a curve on the side which remains illuminated, but such phenomena of direction are themselves on the dependence of most delicate changes in the retina. We must of necessity leave aside this part of the subject, namely, the effect of light on the retinal elements, as it would involve the study of the physiology of the retina, a subject too large to be taken in the short time I have left.

I will conclude this brief description of the effect of light on animals with a few words on changes of coloration. It has been long known that many terrestrial and aquatic animals have the curious power of altering their coloration to take an appearance more like that of the surrounding medium. In this way they are enabled more or less to escape their enemies, or to approach more easily their prey. The researches of Milne Edwards, of Pouchet and of Paul Bert, have shown that in the chameleon the changes in coloration are due to the displacement of pigmented corpuscles of different colours, or chromatoblastes. These corpuscles, which are of a contractile nature, may also modify the colour of the tegument by their dilatation or contraction. The changes in colour may occur by the will of the animal, by a reflex action, and by direct luminous impressions.

Vision has an evident influence on the skin of the chameleon. If the animal has lost an eye, the corresponding side of the body becomes practically incapable of altering its coloration, or at any rate, preserves a shade much lighter than that of the other side. The removal of the other eye restablishes the equilibrium.

When a chameleon is exposed to solar light, its colour becomes darker by the direct effect of light. This phenomenon occurs during normal sleep, during anæsthetic sleep, and even after death. On the back of a chameleon sleeping in the dark, Paul Bert placed very carefully a kind of saddle, cut in a piece of paper. Then, without awaking the animal, he approached a lamp. Very quickly the skin became of a dark brown colour, but on removing the paper saddle, the parts which were covered were seen to have kept their original colour. The most refrangible rays of the spectrum produce this effect with the greatest intensity; red light is practically inactive.

Many animals deprived of eyes, are, however, sensitive to light, and certain animals, the eyes of which have been removed, are equally affected by light. Blind frogs avoid light, and place themselves in the darkest available places.

The proteus (*proteus anguinus*) found in the caves of Dalmatia and Carniole, lives and thrives in the dark. By measuring the length of time elapsing before the creature reacts to different radiations, it has been found that the scale according to which its comfort seems to diminish from darkness, in which it appears to be quite happy, to full white light, which is disagreeable, almost unbearable, is the following: Darkness—red, yellow, green, blue, white. The proteus reacts to any kind of radiation by sudden movements, and at the same time the effect of the light is manifested by the formation of pigment in the most vascular parts of the surface of the body. The researches on the proteus have shown clearly that it is the skin which is acted upon by light in animals, normally deprived of eyes, but it is by a pure hypothesis that this fact has been generalised, and that the existence of such a dermatoptic vision has been admitted in all organisms without eyes.

We have seen that fluorescent substances have the effect of increasing the illuminating power of the violet end of the spectrum and of rendering visible the ultra-violet radiations, which normally are not seen. It follows that a certain quantity of visible light may be produced by spectral radiations. The fluorescent substances existing in animals are, however, very rare. R. Dubois has found in the luminous organs of the pyrophore a fluorescent substance he has called pyrophorine, which appears to serve to increase the light produced by the animal.

The crystalline lens, the cornea, the aqueous humour, and even the retina have been regarded as fluorescent. At any rate, they are very little fluorescent, and were it otherwise this would be more disagreeable than useful for vision. However, the cornea and crystalline lens show a certain degree of fluorescence when they receive ultra-violet rays, and in such cases, as stated by Helmholtz, they emit a pale bluish light similar to that shown by sulphate of quinine solution. The light observed in the eyes of many animals, such as the cat, is only seen in half darkness, never in absolute darkness. The light is probably due to interference of the rays reflected by the tapetum lucidum.

We arrive now to the production of light by living beings. Man found such difficulties in first obtaining fire and its twin brother light, that old legends will have it that a mortal stole it from the sky. Yet it is one of the surprises of nature that numberless creatures belonging to species and families descending lower and lower in the scale of life, can both generate and diffuse light. By means of the light radiating from their bodies generally, or from special organs, they can illuminate the medium in which they live, whether this is the surface of the earth or some subterranean caves, or again the profound abysses of the ocean.

Amongst vegetables, the production of light or, to use a more scientific expression, the photogenic function, or biophotogenesis, has only been observed in plants without chlorophyll, such as mushrooms, and accidentally in parts of higher vegetables deprived of the chlorophyllian function. The family of bacteria includes a great many varieties forming the genus Photobacterium.

Many of those live floating in the sea or on the surface of the body of fishes, crustaceans, cephalopods, etc. It is to those that the phosphorescence of fishes is due, but the luminosity only appears after the death of the fish.

In inferior animals, the photogenic function is diffuse. It is in the protoplasmic mass that the production of light is observed. The noctiluque (*noctiluca miliaris*) produces the phosphorescence of the sea. Mechanical excitations determine the production of light in the mass of these organisms, and it is why the light chiefly appears when the surface of the water is ruffled by the wind.

In abysmal regions, numerous polypes form luminous forests of an appearance reâlly fairy-like, and no doubt it is to this illumination that creatures living at such enormous depths owe their rich colours and the eyes they are possessed of.

Amongst articulated animals, a great many species present the photogenic function to a high degree of differentiation. Many crustaceans are luminous, having on all parts of the body, but especially on the legs, some luminous globules or photospheres, which sometimes are fitted with reflectors and lenses, and have been mistaken for eyes. It may be that such organs serve at the same time to produce light and to see.

But of all animals, insects offer the greatest varieties of luminous beings. The most known are the *Luciole*, the *Lampyre noctiluque*, or glowworm, and the *Pyrophore*, vulgarly named *cucujis* by the natives of West India.

The photogenic function appears in the larva of the lampyre, or glowworm, the luminous apparatus showing as two small yellowish spheroidal masses on the side of the posterior part of the body. The mechanism of the photogenic function is easy to study on the pyrophore noctiluque. As in the glowworm, the egg of the pyrophore is luminous, so is the larva, but in the adult stage the pyrophore possesses three lanterns, two on the top part of the body, at the junction of the head and thorax, and one on the under surface, at the junction of the thorax and abdomen.

The production of light by the pyrophore, like that of the lampyre, is intimately connected with the fluctuation of blood in the luminous organs, and appears to be independent to a great extent of the working of the stigmates which correspond to these organs. If a pyrophore is excited its respiration ceases and its lanterns become immediately illuminated.

If we except the rare cases of luminosity, probably of parasitic origin, occasionally observed in men, the photogenic function in vertebrates is only found in fishes, especially those living in abysmal depths. The photogenic organs may be situated in very different regions, and by their positions and structures they resemble those of the crustaceans. The waters in which these creatures live are so deep that they are far beyond the reach of the remotest ray of sunlight. The sun itself would not appear to these creatures even as a single star of the fifth magnitude. In this region of darkness, some of the living beings have lost their eyes entirely, as might be expected. But others have not only eyes, but very large eyes, some of which are made in a very special manner. As so many of the creatures, including those with eyes, carry light-giving organs, the conclusion is that these animals which spend their life in regions where there is no natural light, are able to see their ways by the artificial light given out by numberless living lamps affixed to themselves and other animals.

A fish dredged from 1,000 fathoms, in the South Atlantic, has eyes so arranged as to be absolutely unique. It is thought that they are modified and arranged on a most elaborate plan, so as to make use of the special class of light emitted by luminous animals.

Another called *malocosteus niger*, a small black fish, brought up from a depth of some 4,500 feet, has two light-projecting organs below its eyes, which probably illuminate its path through the dark ocean as the lamp of a motor illuminates the road. But by far the most striking of these light-bearing creatures is a carnivorous and formidable little fish, called *stomias boa.* From its elaborately carved jaws to the extremity of its body, it has on either side, below the middle line of the body, a double row of lamps or luminous holes, so close together that the luminous plates must light up the water round it far more brightly than the lights of a steamer illuminate the surface waters. It must look like a brilliantly lighted submarine train. This fish has very large eyes and a probably luminous tentacle, like the barbule of a barbel. As no vegetable growths exist in the deep seas, all the creatures living there are carnivorous. Consequently, while the development of light may aid one to find its food, it also betrays it to any others which may probably be hostile. This fact presents some difficulty. In the ordinary light of day, every creature has to take its chances against its enemies. Many, therefore, have become nocturnal, and the eyes of the enemies (as of the cat, the owl, the fox) have been improved in order to catch them. But we do not know whether most creatures in the abysses of the sea are luminous or not. They may be covered with luminous mucus, giving out a strong light at great depths, or otherwise rendered visible; though some which have light producing organs in addition may have great advantage over others in the struggle for existence.

According to the creature which emits it, the colour of the light produced may vary. In the photogenic bacteria it is sometimes white, more frequently bluish, or greenish, or may even have an orange shade. The co-existence of two differently coloured lights has been observed in some cases. Certain beetles have two lanterns giving red light near the head and two others giving blue light in the middle part of the body. But what is more curious still is to see in the same animal all the colours of the spectrum succeed each other without interruption. Such is the case for the pyrosore.

Most of these variations are due to corresponding modifications in the photogenic mechanism. The coloration of the light may also be due to circumstances independent from the method of production, for instance, to the colours of the tegument of the blood. Thus, in injecting some eosine in the blood of the pyrophore, the light originally green becomes reddish.

The light produced by luminous insects shows a continuous spectrum, without bands or lines, and in which the different component radiations are clearly visible. The spectrum of the pyrophore, for instance, is striking when the animal is very luminous. Fairly extended on the red side, it ends at the level of the first blue rays. Its limits may be given as the ray B of the solar spectrum on one hand and the ray F on the other.

The impression produced on our eye by the light of a pyrophore shows clearly that the constitution of this light is not the same as that of our sources of illumination. It is easy to verify this prevision by giving a beam of artificial light, by means of a diaphragm properly adjusted, a photometric intensity about equal to that of the light of the pyrophore, and by comparing the two spectra produced. The graphic representation of the results thus obtained may serve to give an idea of the respective composition of the two lights, but it is obvious that such a method is most imperfect, as it is impossible to compare, by photometric means, two lights widely different in their compositions.

The spectrophotometric analysis, on the contrary, has shown that if curves are drawn taking the wave-length as abscissa and the intensities of the rays corresponding to these waves as ordinates, the area included between the curve and the axis of wave-length is, for the light of the pyrophores, almost entirely occupied by green and yellow rays. The maximum intensity corresponds to the wave-length $0.52856\mu$. Now, this wave-length is precisely that which represents for our eye the maximum brightness in the solar spectrum. In the case of a candle, on the other hand, the maximum of luminous intensity corresponds to a wave-length of $0.48568\mu$, and is therefore rejected on the side of the most refrangible rays. The result would be the opposite if the spectrum of the pyrophore were as it is, simply because of the weakness of its total intensity, because, in this case, the blue rays would be the most abundant. Finally, the area limited by the curve of intensities of the candle and the line of wave-length is only occupied by yellow rays in the narrowest part.

By comparing these areas themselves, it is found that the spectrophotometric value of the two protothoracic lanterns of the pyrophore is about 1/400th of a candle. If we admit, which is about true, that the ventral apparatus has an illuminating power equal to twice that of the prototthoracic ones, we see that we should require about 37-38 pyrophores, giving light by their three lanterns, to get an illumination of one candle.

The light emitted by luminous insects is not polarised, but it contains a fair proportion of chemical radiations in spite of the presence of the fluorescence pyrophorine, which transforms a part of such radiations into visible rays. That the light of the pyrophore contains chemical rays in sufficient quantity to affect a photographic plate, is shown by this photograph of a paper pattern taken by the natural light of a pyrophore. It must be observed that five-minute exposures are necessary to obtain, even with the lantern of the under part of the body, the brightest of the three, a negative with a gelatino-bromide plate, requiring a fraction of a second exposure in solar light.

The amount of heat radiated by the lantern of a pyrophore is infinitesimal, but not absolutely nil. Prof. Raphael Dubois has been able to detect a few calorific radiations by means of a thermo-electric battery of great sensibility, taking all precautions to avoid experimental cause of error. He has found that this small quantity of heat is practically twice that which would be given up in the ordinary way, and at the same time by the body of the animal, when it does not radiate light. The existence of these calorific radiations has been further confirmed by the use of Langley's bolometer, the most sensitive instrument to detect heat in infinitesimal quantities. Langley and Verey estimated in this way that the quantity of heat radiated during 10 minutes by the most brilliant pyrophore is but seven millionths of a calorie.

On the whole the researches of Raphael Dubois have fully justified the conclusion he had drawn as far back as 1886, namely, that whilst in the case of the best artificial light 98% of the energy spent is used to manufacture invisible radiations, especially calorific, and but 2% are used to produce light, in the case of physiologic light, 98% of the total radiations is made of visible light and 2% only are lost.

Besides this immense economic superiority, we have also to consider the exceptional qualities owing to which the pyrophores themselves prefer their own light to any other. This light cannot be put out by wind or rain; with it the danger of fire is eliminated. It must be understood that these little lanterns which the pyrophores always carry with them, which are always available, do not involve a great physiological expenditure. Twenty pyrophores, after having shined many hours, during a period of three days and three nights, have only lost a weight of a little over half a gramme, which represents a loss of three centigrammes, or half a grain per insect, and during that period they had spent more energy in movements than in producing light; moreover, they had not consumed any food.

The practical use of the cold physiologic light in illumination has been shown by Prof. Dubois at the Universal Exhibition of Paris in 1900. To illuminate a vast hall he has employed big glass vessels containing liquid media, in which photobacteria developed, and also glass vessels, the inside walls of which were coated with gelatine inoculated with the same bacteria. He has been able, in this way, to obtain an illumination equivalent to a fine moonlight, and to take photographs by this light.

My assistant, Mr. Firth, has made a lantern slide, showing a photograph of the bust of the great physiologist, Claude Bernard, taken by Prof. Dubois by means of the light just described. Objects viewed in this light appear exactly as they show in the photograph, but an exposure of at least 12 hours is necessary with an instantaneous plate, if a good negative is expected. The reason of this protracted exposure is found in the fact that physiologic light contains a very small amount of chemical radiations.

Here is another slide representing the living lamp of Raphael Dubois, based upon the principle just explained. It consists of a glass vessel with a flat bottom. The upper part is covered with tinfoil, forming a reflector, and the internal walls are coated with a gelatinous layer made luminous by photobacteria. The openings in the neck above and on the side are closed by means of a wad of cotton wool, serving to filter the air passing through the vessel. The circulation of air must be assured by means of an india-rubber ball and tube, especially if the vessel is filled with liquid medium. This lamp may serve as a night light during several weeks, without any interruption.

Mr. Guillaume has tried to find approximately the photogenic efficiency of the electric arc lamp, and has found it to be 1/40; that is, only the 40th part of the energy spent to excite the lamp is utilised to give light. The practical efficiency is still lowered if we take into consideration the amount of energy corresponding to the weight of combustible spent to work the machinery supplying the current, and if we estimate the loss resulting from the different transformations of energy involved. Looking at the question from this point of view, Guillaume has drawn the following table, giving the relative efficiency of different sources of light:—

| | |
|---|---|
| Pyrophore ... ... ... | 1.0 |
| Sun ... ... ... | 0.14 |
| Arc lamp ... ... ... | 0.0025 |
| Incandescent electric lamp | 0.0005 |
| Candle ... ... ... | 0.00014 |

As we see by this, the production of non-luminous radiations is dear, and beside, from the point of view of illumination, the ultra-violet radiations are always harmful and the dark calorific radiations very often are. To conclude, we can see that the physiologic light is produced without the simultaneous production of heat. The practical meaning of the superiority of physiologic light will be emphasised when it is stated that in Great Britain the sum spent annually in manufacturing light reaches, according to Prof. S. Thompson, the gigantic total of from 10 to 20 millions of pounds. Now, at least 99 per cent. of this enormous amount is thrown away in producing heat. It is strikingly obvious that there is room for future development, although again quoting Silvanus Thompson, "the cheapest source of light still remains to be the commonest and most universal, the light of the sun, which shines alike on rich and poor and gives us a light the dominant wave-length of which is just that wave-length to which our eyes have become, in the long evolution of the eyes, the most sensitive. By no artificial process can we manufacture light so cheaply that it would not be still cheaper to adjust our social habits to the hours of sunlight and do our day's work while it is yet day."

Mr. President, ladies and gentlemen, I have endeavoured to put before you this evening only a few facts in connection with the subject, with the purpose, as I said before, of causing some of my hearers to search still further into the secrets of nature.

# THE USE OF MIRRORS AS CONDENSERS.

By S. D. Chalmers, M.A.

*November 14th, 1912.*

The purpose of this note is to call attention to the convenience of using mirrors rather than lenses as condensers for spectrographs and optical testing apparatus.

In the first place the mirror is free from chromatic aberration, and if the source be placed at a distance nearly equal to the radius of curvature there is little or no spherical aberration or coma. The astigmatism which is present is rather useful than otherwise, since a line image is formed of the point source, and by changing the angle of incidence the length of the focal line may be modified as desired.

It is essential that the mirror should be so mounted that it turns about a line on the optical axis of the system.

The convenience of using different sources by merely rotating the mirror is very great. For use with ultra-violet light the mirror should be of speculum metal or nickel on glass.

# ON THE CONSTRUCTION OF ASTRONOMICAL INSTRUMENTS.

By Sir David Gill, K.C.B., F.R.S.

*Presidential Address, delivered December 12th, 1912.*

When your Society did me the honour of asking me to become its President in succession to Dr. Glazebrook, I was not aware that it would be my duty to deliver an address from the chair. I have no experience in the grinding and polishing of mirrors or lenses, nor have I ever devoted myself to original work in mathematical optics. To-night, therefore. I propose to speak to you solely from the point of view of a practical Astronomer and Surveyor, and to relate in a general way my experience with instruments of various kinds, chiefly with regard to the mechanical details of astronomical instruments—and the faults and good points which I have found in them.

The first astronomical instrument of any importance with which I worked was a 30-in. portable transit instrument, by Troughton & Simms, mounted in the usual way on cast iron uprights, and provided with the ordinary means for final adjustment in azimuth and level. It was mounted in the small observatory at King's College, Old Aberdeen; and there, along with the late Professor David Thomson, about the year 1865, I began, as a hobby, to determine the clock error of a good sidereal clock by Dent, and to regulate and keep to mean time a clock which I fitted with contact springs, sending alternating electric currents, to control the Bain-pendulum of the turret clock of the College; in this way I gained my first experience in practical astronomy. The transit instrument was quite a good one, and gave admirable results after I had some little practice with it; I remember well my delight when my eye and ear observations of the seven wires of a transit, when reduced to the middle wire, began to agree with the mean to one-tenth of a second—and when the observations in the two positions of the lamp E and W agreed within a few hundredths of a second of time. But soon all this became routine—the mere keeping of a clock to time was not sufficient for one's enthusiasm, and I longed for means to do practical astronomical work of more interest and more permanent value.

About this time there was an equatorial telescope, by T. Cooke & Sons, of York, which had been made for India, but which, on account of the death of Captain Jacob by whom it had been ordered, was then offered for sale on advantageous terms. There was a sum of money which had long before been left by someone for observatory purposes, and the interest and capital had accumulated sufficiently to provide for the purchase of this instrument; but the existing domes were much too small to cover a telescope of ten or twelve feet focus, and there was no site belonging to the University suitable for the erection of an observatory. Professor Thomson therefore bought a small equatorial by Dalmeyer, of 3½ inches aperture and 4 feet focus, which was for sale at the time. It had been made for display at some exhibition, and its object-glass was admirable; but the stand was much too feeble, the clockwork was utterly inadequate in power and quite unsound in design for uniform running.

Like most amateurs when they first take to astronomy, I tried to measure double stars, but it was impossible with such an instrument to do so with any precision.

It was necessary when communicating slow motion in right ascension to turn a handle that gave motion to the frame which carried the worm screw of the driving circle, and thus communicate a motion of translation to the worm screw in the direction of its axis. This motion, of course, caused rotation of the polar axis. But the slow motion in right ascension so produced had the effect of accelerating the clock or of so retarding it as to even stop it, because by one direction of slow motion the clock was relieved of doing work, and by the other motion its work was doubled. Besides this the inevitable want of fit in the slides was bound to be accompanied by so much "dead go" or "loss of time" in the slow motions that nothing could be more exasperating than the attempt to bisect one of the images of a double star with the fixed micrometer wire, and to keep it so bisected until the image of the other component of the star could be bisected with the other wire. This process was further complicated by the fact that the clockwork was at that very instant in one of its throes of acceleration or retardation, produced by the violent interference with its normal work.

Consider for a moment what is the problem to be realised in such an instrument if reasonable facility is to be given to the astronomer to make such measures. So far as I recollect, the driving circle of this little equatorial was about six or eight inches in diameter. Let us take its radius at 3½ inches or say 87 mm.—then one second of arc will correspond with 4/10ths of a *micron* or about 1/60,000 inch. Now, one second of arc is an intolerably large error in the measurement of a double star—it should be possible with a good telescope of that size, and in circumstances of good definition, to bisect a star's image with at least five times that accuracy, provided that the star's image can be retained at absolute rest, say for a ten seconds of time, and that implies, for an equatorial star, that the driving clock shall rotate the polar axis uniformly within 1⅓ per cent. of true sidereal rate during the ten seconds of time in question.

Now, it is by no means difficult to construct a continuous motion clock, of very simple design, that will not vary more than 1/200th part of its rate, provided that it is not subjected to change of work—but it is absolutely impossible to cut a worm screw and worm teeth with such precision that the worm screw, when turned at a uniform rate, will cause the periphery of the hour circle to rotate uniformly within 1/300,000th of an inch.

For the purposes of double star observing, however, as I have shewn, this uniformity of motion is not essential for more than ten seconds of time—it is therefore only necessary that the clock rate shall be uniformly accurate within, say, 1/200th part, and that the accidental or periodic error of the screw in that period shall not, for a driving circle of 3½ inches radius, change during these ten seconds of time by more than 1/300,000th of an inch. If we suppose the circle to have 720 teeth, that is to say to have a screw rotating once in two sidereal minutes, this would mean that through a range of 1/12th of a revolution of the screw the change of screw-error must not amount to 1/300,000th of an inch.

It must also be borne in mind that this is by no means the sole condition. Besides an accurate screw it is necessary that the driving circle teeth, or at least the side of the tooth against which the screw presses, shall remain in exact contact with the driving side of the worm thread, and that there shall be no intervening dust or ragged part on the tooth. To realise this condition, even in an approximate way, it is necessary that the screw and teeth shall be ground together, and this means, first of all, that the screw shall by some preliminary process be freed from periodic error, and then be made to run the circle round with some very fine grinding material for a long time until the two are harmoniously worked together. The result of this process is, of course, to grind away some part of the screw in its central part. If anyone examines a high-class dividing engine, such as the principal dividing engine belonging to Troughton & Simms, it will be seen that, after the many hundreds of circles it has automatically graduated, this wear or grinding away of the threads of the central part of the screw has become quite evident to the naked eye—but the result is a very nearly perfect adaptation of the screw to the teeth of the circle—as is evident from the small accidental errors (as apart from periodic errors) of the graduations which this machine has produced.

This being so, it is obviously unsound to cause slow motion in right ascension by linear translation of the axis of the driving screw in the direction of its axis (*i.e.*, tangential to the driving circle), because you then at once disturb the carefully ground adjustment of the worm of the screw to the teeth of the circle. Besides—if the driving screw frame is mounted upon a slide—you thereby introduce an additional element of instability of the position of the screw with respect to the circle.

Obviously the most refined and delicate method of communicating slow motion is to give easy means of temporarily changing the velocity with which the screw rotates, without changing the rate of the driving clock itself, and this can be done by the aid of some form of differential gear, such, for example, as that known by the term "Sun and Planet Slow Motion."

Another very bad feature in the design of the little equatorial in question, and of too many since its day, was the necessity to unclamp the worm-screw from the teeth of the driving circle before the telescope could be set in hour angle. This is a barbarity of the worst kind, for in re-clamping the screw it is very easy to injure the teeth by bringing the worm of the screw into contact with the points of the teeth instead of entering at the space between them—and no driving circle clamped in this way has ever long escaped serious injury by deformation of the teeth. It is essential, for the preservation of any driving circle or driving arc, that the worm screw should never be removed from the teeth, that is to say, the polar axis should be free to turn in the bearing of the driving circle and the circle be fitted with means of clamping it to that axis when a fresh setting is made.

But if we suppose the impossible, viz., that this little circle is perfectly cut and its worm screw has no error and perfectly fits the teeth, there is another important condition that remains to be fulfilled, viz., that the telescope mounting must be rigid enough to enable the micrometer to be manipulated without setting up vibration that will interfere with the precision of the measures—or be affected similarly by a reasonable amount of wind.

This want of rigidity has hitherto been a very great fault—particularly in the smaller class of equatorial mountings. In the case of the instrument in question it was quite impossible, from this cause alone, to make good observations, even if the clockwork and other arrangements had been perfect. Indeed, in the great majority of equatorial mountings made previous to 1860 the axes were not nearly rigid enough, and the slow motions in right ascension were far from satisfactory. Thus in observing double stars the elder Struve used to complete his bisection of an image of a double star with the fixed wire by pressing with one hand on the side of the tube whilst with the other he completed the bisection of the other star with the movable wire. One is at a loss whether most to admire the skill by which such difficulties of observation were overcome or to wonder at the want of adaptation of mechanical means to put an end to them. In the matter of rigidity, I think exception should be made in favour of T. Cooke & Sons of York, and Grubb of Dublin, whose mountings were much stiffer than those of the Continental makers at the time. Dawes also had introduced his "slipping piece"* which gave motion to the whole micrometer box in two directions at right angles to each other, and which therefore enabled the observer to bisect one of the images of a double star with the fixed wire without recourse to the slow motion of the telescope either in R.A. or Decln. This made a great advance in the convenience of observing.

But I wander from my subject—I have gone into an analysis of the faults of the little instrument with which I first attempted extra meridian observing, because that led me, very early in my career, to study the conditions of construction that are necessary in an equatorial mounting designed for accurate work.

My conclusion was that it was practically impossible to make the small Dallmeyer telescope fit for accurate work—the only thing to be done was to design an instrument of my own that could be made for a sum of money within my means. An advertisement appeared in the Astronomical Register, in which the Rev. Henry Cooper Key, of Stretton Rectory, Hereford, offered for sale a telescope with a silver-on-glass speculum of 12 inches aperture and 10 feet focal length. This he sent me for trial on a rough wooden stand. I found it gave admirable definition and purchased it.

The next step was to mount it equatorially, and I finally designed a double polar axis, consisting of a strong frame made in a single iron casting. Into this frame was inserted an upper cylindrical pivot resting on rollers and a hemispherical ended lower pivot resting on a hollow hemispherical bearing. The tube was clamped in a central flanged cast iron cylinder. The pivots of this cylinder, which formed the declination axis, were cast along with this central part of the tube, and were truly turned, having axes at right angles to the axis of the tube. These pivots were mounted in bearings attached to the opposite inner surfaces of the double polar axis, and the bearings were so constructed as to bind the two sides of the frame together, so that the flexure of the polar axis, under the weight of the telescope, became nearly the same at the six-hour angle as in the meridian. These parts of the instrument were excellently made, to my working drawings, at the workshops of Messrs. Hall Russell & Co., shipbuilders, in Aberdeen. The driving circle, its tangent screw and slow motion in R.A., as well as the declination circle, were made for me by Messrs. T. Cooke & Sons, of Buckingham Works, York. Slow motion in declination was given by a tangent screw working in teeth cut on the edge of the declination circle. The driving clock was made with my own hands, and consisted of a train of wheels driving a conical pendulum suspended by two pairs of springs, at right angles to each other, on the same general plan as that of Airy's chronograph at Greenwich. A cylindrical pin, projecting from the lower end of the pendulum-bob, fitted easily in a slot cut in an arm of brass, and the latter was attached to the projecting vertical pivot of the train of wheels of the clock-train, and thus communicated circular motion to the bob of the pendulum. The radius of motion was limited by a sliding pin, against the end of which the pin of the pendulum-bob pressed when the desired extent of motion had been reached. At the further end of the sliding pin was an indiarubber finger, which was thus pressed against the inner surface of a brass ring concentric with the axis of the brass arm. This set up friction on the ring, which effectually checked an increase of the arc and prevented acceleration of the period of the pendulum. The whole formed as satisfactory a clock as I have ever known. The pendulum had a wooden rod and was approximately of the length of an ordinary seconds pendulum, so that its period of revolution was two sidereal seconds.

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*As a matter of ease in observing it would of course be better if Dawes' slipping piece had rotated with the position circle, so that one of the slow motions took place parallel to the axis of the measuring screw, the other at right angles to it. In the most modern micrometers the micrometer box is provided with slow motion in the direction of the axis of the screw, and it would be an advantage to give it also slow motion at right angles to that.

I employed the instrument in measuring double stars, examining nebulæ, making photographs of the moon, and generally satisfying my curiosity as to the wonders of the heavens, and I had just begun to plan attempts to determine the parallaxes of some stars with a filar micrometer by Steinheil of Munich, when I received a visit from Lord Lindsay (now the Earl of Crawford). He was then considering the question of creating an observatory at his father's seat, Dun Echt, about thirteen miles from Aberdeen. He had seen some of my photographs of the moon, and desired to examine the means I employed for the purpose. Our acquaintance rapidly ripened, and he became aware of my desire to devote my time entirely to Science. One fine day in 1872 I received a letter from his father, the Earl of Crawford and Balcarres, offering me charge of the observatory which his son was about to erect at Dun Echt.

On acceptance of the post my telescope was purchased from me by Lord Lindsay, and was finally presented by him to the City of Edinburgh at the time when he presented the other instruments at Dun Echt to the New Royal Observatory near Edinburgh. It is now mounted in the City Observatory on the Calton Hill, Edinburgh.

At Dun Echt I had very varied experience. The projected equipment consisted of an equatorial refractor by Grubb of 15 inches aperture and 15 feet focal length; a transit circle by Troughton & Simms, of 8 inches aperture and 8½ feet focus; an equatorial by T. Cooke & Sons of York, of 6 inches aperture and 6 feet focus; a heliometer by Repsold, of 4 French inches aperture and 5 feet focus, exchangeable with the telescope of the Cooke equatorial, so that either one or the other could be used on the same stand; a silver-on-glass reflecting telescope (formerly my own property and already described); an Alt-Azimuth, by Troughton & Simms, with 12-inch circles reading by micrometer-microscopes; a large chronograph, with 4 barrels; a large Foucauld siderostat, with a 16-inch circular plane mirror, to be used primarily with a horizontal telescope of 4 inches aperture and 40 feet focal length; and a portable transit instrument of 4 inches aperture mounted somewhat on the principle of Struve's prime vertical transit at Pulkowa, but provided with two pairs of bearings, so that it could be used in rapidly reversed positions on the same evening either in the meridian or in the prime vertical. In addition to discussion of the details of the working drawings for these instruments, plans had to be prepared for the construction and erection of their respective observatories, domes, etc., and the instruments when completed had to be tested, mounted, and adjusted.

With reference to the equatorial, it was possible to introduce many improvements, some of which were due to the makers, some to my then limited experience. So far as I am aware, the Dun Echt 15-inch and 6-inch equatorials were the first instruments of the kind in which the declination circle was mounted on the same end of the declination axis as the telescope so that it could be read, or the instrument be set in declination from the eye-end.

The 15-inch Dun Echt equatorial was the first in which the driving clock was efficiently controlled by electric currents from a clock with a swinging pendulum.* It is true that the Bonds in America and Thomas Cooke of York had both previously contrived clocks in which the rotation of a fan was controlled by a swinging pendulum. But in either of these constructions a moderate increase of the driving weight will cause the clock to *run up* a certain amount before the control of the swinging pendulum is sufficient to overcome the increased tendency to gain. The effect of such a property is that under variation of resistance or power the image of a star, though retained *near* the micrometer web during long intervals, will be found to oscillate from one side of the web to the other—a property that is fatal in photography and troublesome in all other kinds of observation.

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*A description of this control will be found in the Monthly Notices, R.A.S., Vol. XXXIV., p. 35.

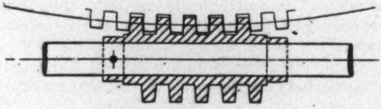
In the new system, whenever the error of the driving clock accumulated to produce an error of about 1/100 of a second of time, the *rate* of the governor was automatically changed so as to correct the error, and then when the error has been corrected the rate of the governor was restored to its normal condition. When the first astrographic equatorials were constructed an improvement was made, viz., not to *change the rate* of the governor balls of the driving clock, but to apply *corrections* to the absolute error by the automatic temporary introduction of differential gear between the governor and the driving screw—that is to say, by altering the rate of motion of this screw, but not of the governor. This is what is now called the Grubb control, but this control depends otherwise on precisely the same principle for detection of the error as that described in the paper in question.

Formerly in the Cooke equatorial, and in those of many other makers to the present day, the form of the teeth of the driving circle (as shewn marked ordinary V in fig. 1) does not seem to me the best possible—viz., teeth cut to fit a worm the sides of which converge forming an angle $\theta$ at the vertex from 45° to 60°.

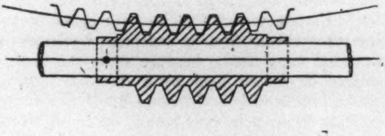
Grubb, and, more recently, Cooke & Sons of

York, employ a worm screw of a totally different kind, or what is called a square threaded screw, fig. 1, in which the worm does not come to an edge, and the angle $\theta$ is very small. It appears to me, for several reasons, that the latter, as shewn in fig. 1, is by far the preferable form.

For a given pressure $p$ between the driving surface of the screw thread and the tooth of the driving circle, the effective part in rotating the circle is $p \cos \frac{1}{2} \theta$, and the force required to press the screw into constant contact with the teeth of the driving circle must be always $> p \sin \frac{1}{2} \theta$. Thus the smaller the angle $\theta$ the more nearly will the whole of the pressure on the teeth of the circle go towards turning the driving circle, and the smaller would be the spring pressure required to keep both sides of the screw threads in contact with the teeth.



- Nearly Square Threaded Worm -



- Ordinary V Threaded Worm -

Fig. 1.

As a matter of fact it is not found conducive to good driving to press the driving screw into gear with the circle-teeth by means of a spring, especially when $\theta$ is large. Thus suppose $\theta =$

60°, then the pressure tending to force the screw out of gear will be $=\frac{1}{2}$ the whole pressure exercised by the surface of the screw-thread on the surface of the tooth of the driving circle. To overcome the irregularities of this pressure the normal spring pressure in forcing the screw into gear must be much increased over its normal amount, and thus a great deal of unnecessary friction is created. More than this, every particle of dust or grit which is carried into the gearing by the screw-thread tends to force the screw out of contact with the teeth, and, if this dust or grit gets to the bottom of the tooth, it forces the screw back, producing thereby not only an irregularity in the driving but a shake or space between the screw thread and its enclosing teeth.

But when the screw is of the Grubb form there is a clear space between the teeth into which dust or grit may go without forcing back the screw; and, even if the screw were forced back, the amount of "shake" (or space between the screw thread and its enclosing teeth) so produced is very small, because when the screw is drawn or forced back by a distance $=d$, the shake produced is $=d\ 2 \sin \frac{1}{2}\ \theta$.

Thus when $\theta$ is very small, as in the Grubb form of screw, it is possible to employ an adjustable stop without a spring to press the screw into its gearing with the teeth, and to grind or polish the thread of the screw and the teeth of the driving circle together so perfectly that no sensible shake remains.

This union by grinding would, if sufficiently continued, bring the centering of the bearing surfaces of the teeth perfectly concentric with the cylindrical bearing on which the driving circle turns, provided that the polar axis itself is not rotated during this process of grinding and polishing. But, of course, in practice the telescope has to be turned through a great variety of angles, and therefore extraordinary precautions have to be taken to insure that the cylindrical (or slightly tapered) bearing on which the driving circle turns shall be absolutely concentric with the axis about which the polar-axis revolves. This has been done by Grubb by taking a fine cut on the circle bearing with a diamond point when the polar axis is rotated on its own bearings.

But I fear that I weary you by this long story of the process of evolution. It will probably be more interesting and useful if I proceed at once to describe an equatorial mounting for a telescope of 26 inches aperture and 35 feet focal length, now nearly completed by Sir Howard Grubb for the Union Observatory, Johannesburg, which contains nearly all that experience has taught us in the matter of the modern equatorial mounting.

Dealing in the first place with the question of rigidity: We have first of all the strong iron castings which rest upon the masonry pier. These form at once the support for the bearings of the polar axis, and the enclosure of the driving clock. The polar and declination axis are exceptionally massive. The upper pivot of the polar axis has a diameter of 16 inches, and the pivot of the declination axis next to the telescope tube is of 12 inches diameter. Both axes are of cast iron, the pivots are surfaced with thin hollow hard steel cylinders shrunk upon the iron pivots. The cast iron central part of the tube is bolted to an end block, which is cast as part of the declination axis. The tubes are formed of rivetted steel plate from ¼-in. thick at its flange-attachment to the central tube, to 1/10th in. thick near the objective and eye-ends, and are preserved in cylindrical or conical form by strong internal rings of great rigidity of form. These dimensions convey some idea of the rigidity of the structure as a whole. But there are other elements necessary to produce astronomical rigidity, by which I mean the accurate retention of the image of a celestial object in bisection by a spider web or upon a definite point of a photographic plate or spectroscopic slit.

This implies, first of all, that, in spite of necessary manipulation in the process of observing or setting, and in spite of a reasonable amount of wind, no sensible vibration shall be set up. But this is by no means all; it is besides necessary that the controlled motion of the clockwork shall be conveyed directly and uniformly to the moving telescope, without jerks of the most minute kind, due to deformation or flexure of the driving circle, inequality in the motion of the screw, flexure of the screw mounting, or torsion of the polar axis. Now the moving parts of the telescope weigh in all 5½ tons, and the telescope tube, object glass, eye-end, counter weight, and declination axis together weigh three tons.

The force required to move such weights on pivots of 16 and 12 inches diameter would be very considerable, and would certainly involve not only much inconvenience to the astronomer but such friction on the teeth of the driving circle, and such flexure of the driving circle and supports of the driving screw, as would make smooth motion impossible.

But the use of ball bearings for relief of friction has entirely removed the difficulty. A strong double lever (part of which is shewn at L, figs. 2 and 3), is adjusted by the screw S, fig. 2, and presses up the ring B, containing steel balls, against the upper pivot of the polar axis in a direction at right angles to the axis itself, and so relieves the pressure of this pivot on its bearings. The friction of the end-thrust of the axis is entirely taken by ball bearings.

The relief friction in case of the declination axis is necessarily a little more complex, but no less efficient. A bracelet of the form shewn in fig. 4 is fitted on the large pivot of the declination axis, and separated from it only by steel balls. The tube and declination axis, when the latter is horizontal, balance on this ring.

If a pin fixed on the crosshead of the polar axis passes rather loosely through the hole *b*, and the end of a lever, having a joint at *c*, passes through the hole *a*, then the counter weight W, fig. 2, can be adjusted so as to balance

Fig. 2.

the whole weight of the telescope and declination axis, when the latter is horizontal. Not only so, but into whatsoever direction the crosshead is turned, the resolved effect of the weight of the telescope and declination axis will also be relieved. That is to say, the pressure on the bearings of the pivots of the declination axis will be nearly zero in all hour angles. The end-thrust of the declination axis is, of course, simply limited in both directions by ball bearings.

In this way both in right ascension and declination there is such perfect ease of motion that

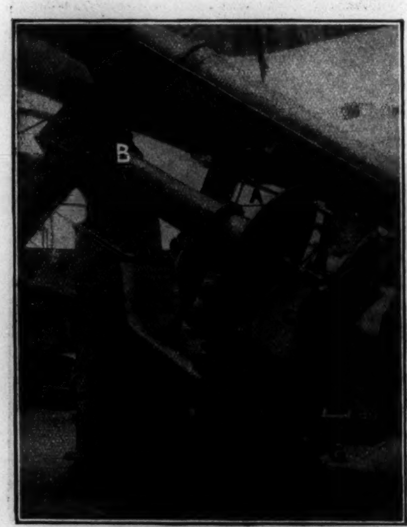


Fig. 3.

the whole instrument can be moved by pressure of one finger.

There are many points of great practical convenience in the structure of the instrument.

Thus, for example, in fig. 3 we see the very rigid driving circle A of 4½ feet in diameter, which, along with the driving screw in the completed instrument, is entirely enclosed in a metallic dust proof cover. The hour circle C (figs. 2 and 3) of 18 inches diameter, fits loosely on the projecting toe of the polar axis, but is moved forward at each second of time by a magnetic clutch acting on a worm screw that gears into the toothed periphery of the circle. This motion is communicated by galvanic signals from an ordinary sidereal clock fitted with the necessary contact springs; the motion of the hour circle is entirely independent of any stoppage of the driving clock or of any slow motion communicated to the driving circle.

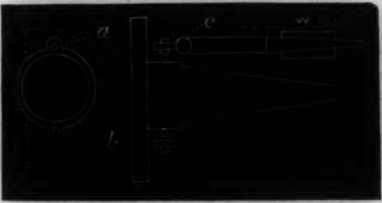


Fig. 4.

This circle, once set, will always show sidereal time so long as the controlling clock sends true signals at each sidereal second. Thus, if an arm carrying an index or vernier is firmly attached to the end of the toe of the polar axis, the telescope can be set directly to the right ascension of any object without the necessity for computing the hour angle.

But in the present case the observer is on a rising floor, and may be a considerable distance from the toe of the polar axis; it is therefore desirable that he should be able to set or read and to clamp the telescope in R.A. at any position of the rising floor. This is accomplished as follows:—

Prisms of total reflection *a* and *b*, fig. 5, are attached to an arm that is permanently fixed to the toe of the polar axis. An image of the graduations of the hour circle is formed by the lens *c* on the cross webs *d*. These webs are in the principal focus of the lens *e*, the diverging rays from *d* being reflected from the plane mirror (or reflecting prism) *f*, at such an angle that the pencil of parallel rays from *e* proceeds vertically downwards to the bent telescope *g*. This bent telescope is mounted on a vertical slide attached to the telescope pier, and moves up and down this slide as the floor of the observatory rises or falls, so that its eye-piece *h* is always situated in a convenient position for use by the observer.

Below the hour circle in fig. 3 is shewn the grooved wheel E, over which passes an endless rope by which the observer, at any height of the floor, can turn the wheel E, and it, by intermediate gearing, turns the wheel F, which is rigidly attached to the polar axis. In this way the telescope can be readily set in hour angle. To clamp in hour angle there are two cords hanging down from pulleys (one of these pulleys is shewn at G, fig. 3). These cords pass through the tube H, fig. 3, and round the revolving rings on the polar axis which communicate by internal gearing with the mechanism by which the hour circle is clamped to the polar axis. The clamping spindle in R.A. can also be turned from the eye-end of the telescope.

By aid of a little electro motor acting on the differential gear, an independent motion for more accurate setting in R.A. can be communicated. A still more delicate slow motion in R.A. is given by Grubb's usual plan of stopping by an electro-magnet one or other of two wheels of the differential gear, one of which accelerates, the other retards, the speed of the driving screw. Throughout the whole design of the instrument regard is paid to the fact that a modern equatorial has to be designed for a great variety of purposes, and has to carry auxiliary apparatus of great variety of weight.

Thus, for example, in the Cape equatorial the

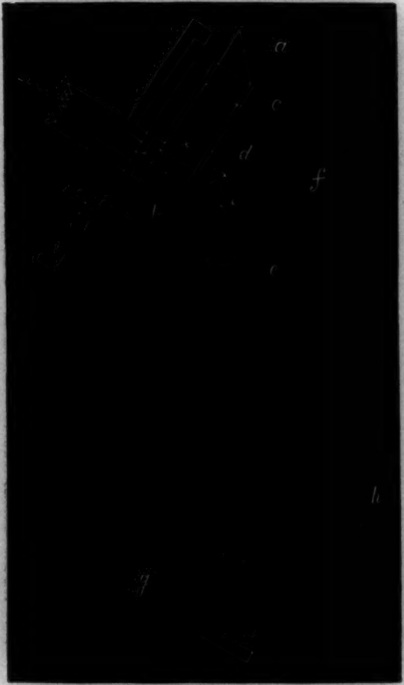


**Fig. 5.**

spectroscope weighs 400 lbs., and that now under construction for Dr. Ristenpart at Santiago weighs 300 lbs., whilst the filar micrometer, or the dark slides for direct celestial photography, are comparatively light. There is but one convenient method of dealing with the alternative use of these different appliances, viz., to balance the equatorial with the spectroscope attached,

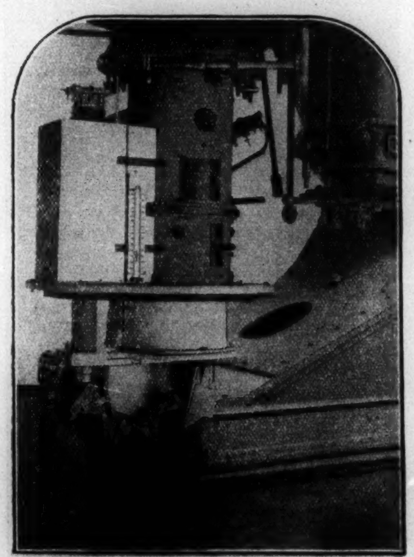


Fig. 6.

and when the lighter pieces, such as the micrometer or photographic slide, are used, to add additional weight to the eye-end. As the requisite definite counterpoises are provided, together with simple means for their attachment, the process of exchange becomes very simple, and involves no change of counterpoises either at the objective end or at the further end of the declination axis. The spectroscope, when mounted on a suitable stand, is easily brought to its proper position for attachment to the telescope tube by means of the rising floor.

A spectroscope for line of sight work should be of very refined construction.

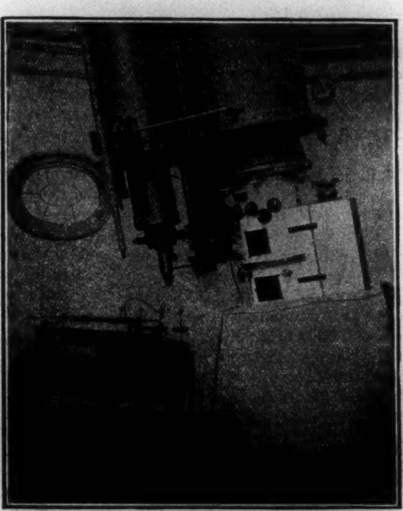


FIG. 7.

The essential conditions are:

(1) Perfect mutual rigidity of the parts, notwithstanding changes of position and external temperature.

(2) Conservation of constant temperature of the prisms and of the supports of the camera telescope.

(The interior of the spectroscope must be kept at a uniform temperature not only during the time of exposure of the spectroscope, but for a number of hours beforehand—because glass is such a bad conductor of heat that some hours are required to bring even moderately sized prisms to uniform temperature throughout.)

(3) Perfection of definition on the plate for all rays employed.

The spectroscope of the McClean telescope at the Cape was designed for determining the velocity of stellar motions in the line of sight—if possible, with an accuracy sufficient to determine the solar parallax within $\pm 0''.01$, or say within 1/1,000 part.

Fig. 6 shows this instrument attached to the McClean telescope at the Cape, the enveloping cover for preserving uniform temperature,—the cover of the prism box being removed.

Fig. 7 shews another view of the instrument with the enveloping cover applied.

It is evident that when our earth, in its revolution round the sun, is in quadrature with respect to a star situated on the ecliptic, the earth (that is to say, the observer) will be approaching to or receding from the star with a velocity equal to the velocity of the earth's motion in its orbit.

Thus, in fig. 8. If S is the sun, $E_1$ and $E_2$ the positions of the earth (in quadrature) at two epochs six months apart, and if the direction of the earth's motion round the sun is shown by the arrow heads, it is clear that, if the earth revolves in a circle round the sun, the velocity of approach of the earth to the star will be greater when the earth is at $E_1$ than when at $E_2$ by twice the velocity of the earth's motion in its orbit; and this *difference* will be independent of the existence of any other uniform velocity of our sun with respect to the star.

Therefore if one knows the velocity of the motion of the earth in a circular orbit and the time required to complete that orbit, we determine at once the circumference of the circular orbit, and therefore its radius—that is to say, the earth's distance from the sun. Of course, the earth's orbit is not circular, but its eccentricity is known, and therefore the relative velocity at

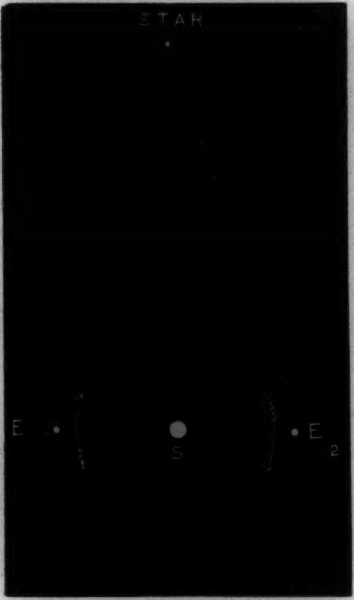


Fig. 8.

any point of the orbit to its mean velocity is also known—so that no uncertainty is created in the determination from the eccentricity of the orbit. The same may be said of the changes of the earth's velocity in its orbit by the action of the moon and planets. But even with the four prisms of the Cape spectroscope and its camera telescope of 30 inches focus, the determination of the velocity of the earth's motion within 1/1,000th part of its amount involves a very small linear displacement of the lines of the spectrum, viz., about 1/10,000 of a millimeter,—a quantity that is only determinable from the mean of a great number of measures on many different plates. But this statement is sufficient to show what extraordinary precautions are required to preserve the relative rigidity of the parts of the spectroscope during the exposure of an hour or more.

The Cape spectroscope was made by the Cambridge Instrument Company on lines laid down by me, with some elegant additions due to Mr. Horace Darwin. The rigidity of its construction is perhaps best illustrated by the portion of the new spectrograph here exhibited, which is now under construction, for Dr. Ristenpart, of the Santiago Observatory, by Messrs. Hilger, to designs worked out under my supervision, on lines very similar to those of the Cape spectroscope. The principles that underlie the designs both of it and the Cape spectroscope are:

1. The minimum number of parts, all of great strength.

2. A construction based on sound geometrical principles, having regard, in all the fittings, to stability depending only upon the necessary degrees of freedom. Thus, for example, the ordinary tube for focusing, sliding inside another tube and clamped by a split ring closing upon an outer tube split at the end, is quite an unsound plan for the most refined work. In this spectroscope all the tubes are of solid drawn steel, turned true where they slide in bearings. These two finished cylinders on the tube, slide each in three well-fitted bearings, 120° apart; but, in clamping, a well fitting ring on each cylinder, close to the bearings, is drawn up by a screw pressing the tube against two only of the bearings of each cylinder. This insures true

geometrical rigidity—for both cylinders of each tube are clamped in this way simultaneously by a single screw, so that the tube rests in bearings like those of a transit instrument. The prisms are mounted (without adjustment) for minimum deviation of the ray H $\gamma$, which ray is thus photographed in the centre of the photographic plate. All the prisms are mounted on a single cast iron plate, and held in position by springs pressing them against the minimum number of stops requisite to limit the freedom of each; thus no prism can be in undue constraint. The adjustments are made once for all simply by scraping the stops.

The removal of two of the prisms enables a mirror, which permanently remains in place, to reflect the dispersed rays from two prisms instead of four into the camera telescope. Thus the spectroscope can be employed with two or four prisms at pleasure, and with one or other of two cameras which may be of different focal length.

3. Another condition is the necessity for preserving the instrument at constant temperature, notwithstanding all variations of external temperature. This is requisite not only because the deviation and the dispersion of the prisms are affected by changes of temperature, but because the constancy in parallelism of the axis of the camera telescope with that of the collimator depends on the relative constancy of temperature in the two supports of the camera telescope. This renders the use of a thermostat necessary. One may use for this purpose a large bulbed thermometer into the bulb of which a platinum wire is soldered in contact with the mercury, and into the capillary tube of which is inserted a fine platinum wire that completes an electric circuit when the mercury rises in the capillary to make contact with the thin platinum wire.

The electrical circuit is completed through a relay that cuts off the heating current when this contact is made. The heating current passes through coils of wire in an enclosing cover which includes the supports of the camera telescope and the prism box. It is essential to provide a powerful fan or fans to thoroughly circulate the air throughout the whole, otherwise there is great liability to accumulation of heated air in the upper part of the enclosure and to difference of temperature in different parts of the apparatus.

In the Cape spectroscope a still more efficient thermostat is provided by means of the changes of electrical resistance of a bridge of which one arm is of copper the others of an alloy that does not change its electric conductivity by temperature. A full account of this thermostat will be found in the "Description of the Cape Observatory," which will shortly be published.

The results obtained by Dr. Halm with the Cape instrument have been eminently satisfactory. The values of the solar parallax derived from two independent series of spectroscopic observations agree within 1/2000th part not only with each other but also with the results obtained from heliometer observations of minor planets and from the international photographic observations of Eros.

There is a hardly less important adjunct of the equatorial, the eyepiece micrometer.

I exhibit here one made by Messrs. Repsold for the new observatory at Johannesburg. It embraces all that is practically necessary, and illustrates very well the advantages of the independent motion of the micrometer box to which I have referred.

The perfection of the methods of illumination and the general arrangement of the instrument exceed in convenience anything hitherto made in this country.

I have worked, first and last, with many varieties of theodolites, and one might easily devote an address to discussion of their various merits. I must content myself with a description of that which in my experience has proved the best.

For general work of high accuracy I have found nothing to exceed in precision and convenience the 10in. Repsold theodolite now shown on the screen.

In astronomical observations with a portable instrument it is a great convenience to be able to observe stars at all altitudes whilst looking in a horizontal direction—as one can do with an instrument having a prism of total reflection in the axis—*i.e.*, on the system known as the broken transit.

The eyepiece micrometer can be accurately turned through 90°, so that the instrument can at pleasure be used as a Talcott zenith telescope for determining astronomical latitudes; it obviously also can be employed as a transit instrument in the meridian or any other azimuth, or for meridian or extra meridian altitudes, or for all the processes of geodetic triangulation.

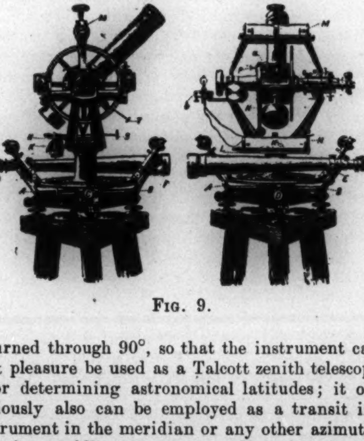


FIG. 9.

In operations of the latter class the use of the eyepiece micrometer is important. The image of the distant object is often so unsteady that a single pointing may give a result very largely in error, for even in favourable atmospheric definition the readings of the circle microscopes are more precise than the single pointing on the distant beacon or heliostat. It is therefore always advisable to make a number of pointings with the eyepiece micrometer on the distant object for each reading of the circle microscopes—and in circumstances of very unsteady definition ten or even twenty pointings may be read. It is quite astonishing how accurate the mean of apparently widely discordant observations of this sort proves to be, *provided* that the observations are taken at regular intervals as quickly as possible one after another, and that no attempt is made to improve the observed results by the rejection of apparent discordant readings.

In geodetic surveys of country where the conditions of time and money forbid the transport of cement and the cost of building masonry piers, it is essential to have an easily transported stand—but the absolute rigidity of such a stand cannot be relied upon. In such a case the watch telescope attached to the supports of the microscopes is an invaluable addition, as simultaneous readings of the micrometer of the watch telescope with those of the eye-piece micrometer entirely eliminate the effect of change in the azimuth of the feet of the theodolite produced by twisting or other disturbance of the stand.

The flint hard steel pivots of the instrument and their permanent perfection of form, are vastly superior to the bronze pivots too often employed even in otherwise high-class theodolites.

The limits of available time prevent me from going into further details with respect to the instruments already mentioned. It might have also interested you if I had discussed the results of my experience with the heliometer and transit circle, and the improvements which modern experience has suggested in their construction; but these are matters which must be deferred to another occasion.

**Mr. Chalmers** desired to express the thanks of the Society to Sir David Gill for his presidential address, and to ask him, on their behalf, to allow its publication in the Transactions of the Society. Sir David had dealt with a subject which was a little outside their usual scope, but he considered it was a very great advantage to them, now and then, to be given the opportunity of travelling somewhat beyond the bounds by which they were ordinarily circumscribed. The apparatus described by Sir David Gill was much larger than most of them were accustomed to, and the problems discussed by him were problems which differed somewhat from those with which they were accustomed to deal. They differed somewhat in type, but not altogether in character, because they were all troubled with the same difficulty—the difficulty, namely, of making things stand still whilst under observation; and they were able, therefore, to some extent, at all events, to appreciate the great skill manifested by Sir David in overcoming those difficulties. They could appreciate, too, that the same methods which had been found so useful in the larger apparatus employed by Sir David Gill would be equally successful with their smaller apparatus if applied with equal skill. He was sure everyone present would join with him in thanking the President for his extremely interesting address, and for the opportunity he had afforded them of seeing how the best work in connection with astronomical observations had been done of recent years.

**Dr. Rosenhain** said it was his pleasant duty to second the vote of thanks for the presidential address, and in doing so he might say that he had listened with very great pleasure and interest to that address. It was indeed quite a rare pleasure to sit there and listen to the story of the experience of a lifetime in a subject of such great interest as astronomy. Such an experience, he thought, appealed with very great force to those among them who were in any way accustomed to deal, either by way of manufacture or use, with scientific instruments of precision of any kind. The subject was one which he thought was neglected far too much, and it had been a perfect revelation to him to-night to see how splendidly the many difficulties had been met in the case of astronomical instruments. The contention had been many times advanced that a scientific instrument was really a machine, and should be constructed like a machine in order to do the work of a machine, and when it came to dealing with a weight of five tons, as in the case of the telescope of which Sir David had shown them a model, it was very obvious how necessary it was to have the most careful attention paid to the details of construction in order to ensure the necessary accuracy. It occurred to him that strangely enough there was a wonderful analogy between these great telescopes and another type of instrument of, unfortunately, a less humane character, namely, the large guns used in modern warfare. There really was a very striking analogy between the devices adopted by Sir David Gill, and by astronomers generally, in their great observing instruments, and those employed by artillerists in the enormous weapons of the present day. In the latter, it was true, they had to deal with weights, not merely of five tons, but 80 and 100 tons, but, on the other hand, they had to be content, in the case of those great guns, with a degree of precision nothing like so high as was necessary in the case of instruments intended for astronomical purposes. This analogy was a very interesting one to notice, and anyone who cared to look into the subject would see the striking analogy which existed in the methods adopted in the two cases.

Another thing which occurred to him whilst listening to the address was, that with all the care, ingenuity, skill, and knowledge displayed in designing these large and beautifully-constructed instruments, one was inclined to doubt whether the choice of materials had always been quite the wisest. For instance, Sir David Gill had spoken of a cast-iron axis with hardened steel shrunk ends. Well, from all the knowledge one had of the qualities of steel, he was inclined to doubt the permanence of adjustment and accuracy of those shrunk ends. Very hard steel had a certain element of non-permanence, and in addition to that, in the case in point, a severe mechanical strain was put upon it in shrinking it, and thus the element of instability was to some extent increased, and he should not be surprised to find, in the course of a few years, these arms had changed their shape quite perceptibly. If that were so, it was a pity, because he thought that particular trouble could be avoided by the use of other materials. The modern metallurgist had placed at the disposal of instrument-makers materials of which the latter knew very little, but which, in many cases, would undoubtedly be very useful to him. Then with regard to the weight of these great telescopes—for instance, the weight of the very beautiful instrument Sir David had shown them that evening, it was a fact that by the use of a light alloy of aluminium it would be possible to obtain equal strength with one-third the weight or three times the stiffness, simply because the actual bulk of the metal for equal weight was very much larger, and therefore its moment of resistance was correspondingly greater. Of course, there might be objections to the use of such alloys of which he had no cognisance. It was not open to him to discuss the presidential address—(The PRESIDENT: I wish you would)—but he hoped he would be excused for having ventured to mention one or two matters which had occurred to him during the course of that address, the vote of thanks for which he had great pleasure in very cordially seconding.

**The President** said he was very much obliged to his audience for their kind reception, and particularly to the speakers for their complimentary remarks. Chiefly he felt himself indebted to Dr. Rosenhain for his observations, which were most suggestive and most useful. With regard to the shrinking of steel pivots, he had no doubt it was possible to imagine the ill effects which Dr. Rosenhain had indicated, but the shrinking on of these hardened steel pivots was not a matter of very recent practice, and it had stood the test of examination for a very long time. The pivots of focused instruments so made were examined from time to time, with a view to determining their absolute errors, and they did not seem to change. He did not know whether they had re-examined the pivots at the Cape since he did so when he was out there, but he would ask Mr. Hough to have them re-examined and see whether any changes had really occurred. Dr. Rosenhain's suggestion of another alloy for large spectroscopes and other similar instruments was one of immense value, and he would be very glad indeed to have a private talk with Dr. Rosenhain on the subject if he would be good enough to afford him an opportunity. What alloy would he think preferable?

**Dr. Rosenhain** said the choice was between several. It would largely depend upon whether the metal was to be in castings or not. Perhaps aluminium-copper would be best.

**The President** said that aluminium-copper had been already used in the case of a large theodolite now at the Cape, which certainly was lighter than if it had been made of the ordinary gun-metal that was generally employed in making these articles. But no doubt since the time that instrument was made great advances had been made in our knowledge of the properties of alloys, and he would be very glad to hear further particulars with regard to this matter. Their temperature co-efficient, no doubt, would be much higher.

**Dr. Rosenhain**: .23.

**The President**: As against .16.

**Dr. Rosenhain**: On the other hand, they are much better conductors of heat than steel, and in a waterproof vessel would retain the heat of the vessel longer.

**The President**: There is no water in this case; it is air.

**Dr. Rosenhain** said these alloys would adapt themselves much more quickly to changes of temperature.

**The President** replied that they did not care about the quickness; what they wanted was to keep a uniform temperature for a long time, and they maintained an equable temperature by automatic regulation of an electric current passing through coils of wire. If he could have got the spectroscope in question made of "invar" he should have preferred it to anything else, but it was such a horrible alloy to work that all the engineers fought shy of it; it cut their tools up, and caused them to use very bad language and charge very high prices. "Invar" was troublesome for some purposes because it was subject to all sorts of secular changes, but even that would not matter in a spectroscope, because the secular changes would be imperceptible during the short time of exposure of a plate. He was very grateful for the kind reception they had given him, and he would certainly take the earliest opportunity of putting his address into a printable form, which he was sorry to say it was not at that moment.

# ON THE MINING, PREPARATION AND USES OF MICA.

By Harold P. Wiggins, Associate I.E.E.

*Paper read February 13th, 1913*

When your learned Society in a rash moment suggested that I should read a paper before you on mica, I found myself immediately confronted with two difficulties, firstly, my total inexperience in writing up and reading a paper on anything, for which I must claim your indulgence, and secondly, Dr. Silvanus Thompson lectured to you so brilliantly, and in such a fascinating style, on the optical and physical properties of mica, only a few years ago*, in which lecture he covered the ground thoroughly in his inimitable way, that it was with considerable misgivings I was persuaded by my friend Mr. Hughes to put before you a more commercial and general aspect of this wonderful crystal.

Mica is known to have been an article of commerce from very early times. The ease with which it could be split and cut into thin transparent plates, and its resistance to fire or easy breakage, must have attracted the early attention of the primitive commercial man. Its dissimilarity to any other known mineral, and its often peculiar mode of occurrence, have given rise to the most poetical ideas concerning its origin amongst the natives of countries where mica is found.

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*Optical Society, November 28th, 1901. Recently re-published in The Optician, October 25th, 1912.

Hindu writers imagine the crystal to be the remains of lightning flashes from which sparks had emanated and become preserved in the ground, and were accustomed to regard this mineral as being endowed with extraordinary properties. Even now in India the miners regard the "books," as the crystals are called, as allied to a sort of fungus growth, a belief that is fostered by the discovery of surface crystals in ground eroded by the heavy rains. From the earliest times large quantities of this mineral have been mined there for medicinal purposes, and even the most deadly diseases were supposed to yield to its healing powers—which seems to show that there is nothing new about the modern cure-for-everything patent medicine. For this purpose the black iron—mica-biotite—is used by the Hindus. According to Sanscrit writers it is first purified by being heated and washed in milk, the plates are then separated and soaked in various juices for eight days; it is then reduced to powder by being rubbed up with rice within a thick cloth, the powder passes through the fabric and is collected for use. This is one form called "*dhanpabhraka.*" It is further prepared by being mixed with cow's urine and exposed to a high temperature for a hundred times, sometimes this is repeated for 1,000 times, and is sold at about R. 8 per *tola.** Mica thus prepared is a powder of brick dust colour and a saline earthy taste. It is considered tonic, and the dose is six to twelve grains. The Chinese believe mica to have the power of prolonging life.

Some of the Indian mines date back certainly to the Christian era, and probably to much earlier times than that. The powdered mineral was spread over the amphitheatre in Rome to obtain a brilliant glistening effect as the dust rose in the sunlight. In India it was—and still is—used at native festivals, marriages, and in the Mahommedan "*maharam*,"[*] for processional ornaments, such as lamps and tinsel decorations on fans, in temples and palaces, and so on. The powder is used for ornamental pottery, on curtains and cloths; in calico printing, and by the washerwoman, to give a sparkle to cloth to which the fine glistening particles easily adhere; large numbers of pictures and portraits are painted on mica-sheets of various sizes. Here are some examples, two fruit dishes, which have been kindly lent to me, ornamented by painting on mica, and three paintings beautifully executed of a bird, a butterfly and a dancing girl on three separate thin sheets.

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*About £30 per lb.

A certain kind of silvery mica is highly prized by the natives for inlay work. The lanterns used by Columbus on his first voyages were protected by mica in place of horn. Thales[†] knew of the mica deposits in India, and I am told that allusion is made to these mines in the Koran.

Mica was also used by the North American Indians in early times for ornamental purposes, as is evident from the finding of mica plates and ornaments in the tombs and mounds of the mound builders hundreds and perhaps thousands of miles from its place of origin—if, as now believed, all this mica was exported from a single centre in North Carolina. Here it was that in 1869, under the shadow of Mount Mitchele—the highest point east of the Rockies—when exploring one of these ancient and mysterious shafts in search of antiquities, that the explorers re-discovered their real object—mica; thus solving an archæological problem and at the same time discovering what proved

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[*] The great Mahommedan festival which commemorates the death of the grandsons of the Prophet.

[†] One of the seven wise men of Greece, born about 644 B.C., and died about B.C. 548.

to be one of the richest mica mines in the United States. Anyway, sufficient has been said to show that the mica trade may legitimately claim to be one of the oldest of the minor trades. Prior to about the year 1880, the largest use of mica was for closed stoves, such as are used for anthracite and coke. About that time its valuable properties as an insulator began to be known. Edison was one of the first, if not the first, to use it for insulating the commutator segments and armatures of his dynamos, and the demand rapidly became so great as to outstrip the supply, the output of the mines was doubled, trebled, quadrupled, fresh sources of supply were opened up; then came over-confidence, over-production, and lean years, many mines were closed because they no longer paid to work, legal and other complications ensued, until the world's total output of mica was in 1909 only one-third of that in 1907. The demand with the revival of trade has become greater than before, with the result that to-day mica has reached a value which makes it rank with the more precious metals.

Now, when one considers that mica is one of the most common and widely distributed minerals that make up the earth's crust, it is not readily apparent why it should sell in large quantities at so high a price, and perhaps for this reason it is worth while considering its occurence and the peculiar conditions which influence its mining and preparation for the market.

How universal mica is may be observed by anyone, as its glistening nature proclaims its presence in granite, gueiss, schists, and similar rocks, and in some places the ground is literally covered with it. Its two usual associates are quartz and felspar, these three combining to form the rock familiar to you all as granite. All three are the products of the earth's heat, and are characteristic of metamorphic and igneous rocks. The coarseness of the rock thus formed is in direct ratio to the rate of cooling, only in the case of igneous rocks (that is, where the material has been completely fused) are the individual crystals of mica, quartz, and felspar found in any considerable size, and then only when the cooling has been with extreme slowness—has been extended, we may suppose, over many years, even centuries—so as to allow each crystal to grow by accretion, and the different minerals to crystallise in order as their respective points of solidification are reached. An examination of almost any piece of granite seems to show that the felspar and mica crystallise out first whilst the quartz is still molten and fills up the spaces between them. Thus we arrive at the gigantic form of granite called pegmatite, in which most of the mica of commerce is found, and which is still made up of mica, quartz, and felspar, but in crystals varying from the size of one's hands up to that of a small house. But while in such formations both quartz and felspar occur in mountain masses individually, mica is found only in comparatively small crystals or clusters of crystals, associated with other minerals. Not even all pegmatite contains mica, much of it is made up of quartz and felspar only, and even when mica is present it occurs with the greatest irregularity and uncertainty.

The pegmatite occurs in what are known as "dikes" by the geologist, and "veins" by the miner. They intersect the strata at all angles and in a very erratic fashion, running hither and thither, branching suddenly, widening and narrowing, and at times disappearing altogether, perhaps to re-commence again in a nearly parallel vein a few feet further on. On the surface of the ground they are scarcely ever continuous for more than a few hundred feet.

But the discovery of a pegmatite "dike," even in a known mica district, gives no indication of the presence of mica. For the most expert miner it is still to all practical intents and purposes mere guess work whether paying mica is present or not, and no certain indications of the presence of mica in a dike have yet been discovered. Sometimes there is a distinct lead which the mica blocks follow strung out along either the hanging or a foot wall of the dike, and in this case the excavations may be made through the somewhat softer country rock instead of the hard pegmatite. But this lead is often discontinuous, running into worthless scrap, and may or may not run into remunerative quantities again further on. Thus days may be lost looking for a new lead, and it often happens that the entire profits of a rich haul of mica are lost in this way. Mines have been abandoned as exhausted, and yet when again worked by a subsequent owner have yielded plentiful returns. A good instance of this is the Lacey mine, which is the largest producer of amber mica (phlogopite) in Canada to-day. It was opened in 1880, and acquired by the present owners, the General Electric Co., of Schenectady, in 1894. This company did not commence work at once, but leased it to another firm for five years, the leaseholders ceased work on it some time after as unprofitable when within a few feet of the immense deposits which have subsequently been worked by the present owners. One crystal taken from this mine is said to have been over nine feet in diameter, and the daily output at one time to have been between four and five tons of rough mica.

The leads are not always present, for the mica often occurs bunched together in pockets, in great agglomeration of crystals, or even single crystals of large size.

Because of the impossibility of correctly or even approximately gauging the persistency of the mica leads, or what is even more important, the quality of the mica they carry, mining in all parts of the world is usually done in a very primitive manner with a minimum of plant and initial outlay, the chief purpose being to extract all the mica possible with the least expense, and this is most frequently done by open quarrying. In cases where the mica occurs not only in pockets, but also disseminated throughout the mass of rock, this method is probably the best, but when the mica follows leads this system is very unpractical, since it is then often necessary to remove large masses of rock, occurring between the veins, entailing much extra and unnecessary labour. An outcrop of mica having been located, a pit is sunk at the spot, often without any preliminary trenching being undertaken to ascertain the nature of the deposit, its general direction, whether it is of the pocket and fissure, or the contact type, or if a better position for sinking cannot be found. Such details seldom seem to worry the operator, who puts down his pit in haphazard fashion and continues work so long as he sees paying quantities of mica, or until forced to quit by the accumulation of water in the pit or the caving in of the sides as they are undermined.

The largest mines are in India; in one district alone some 6,000 men and an indefinite number of women are employed, but the open quarry system is still employed, and the methods generally are probably the same as were employed some 2,000 years ago. The vein of pegmatite is followed sometimes to depths of 200 feet or more, by means of inclines in zig-zag fashions. At intervals along these inclines, native women are stationed, who pass baskets of mica, or pitchers of water, from hand to hand, sometimes instead of inclines double ladders are used. Two rows of women are usually employed from the water level to the surface, the full pitchers being passed up one line and the empties down the other; as many as seventy women are employed in this style of work alone. On a few very rich deposits an inclined zig-zag form of tunnelling is used instead to follow the vein; in this case circular shafts, about two feet in diameter, are put down at frequent intervals along the course of the vein for ventilation and for the raising of waste. Work is conducted only during the dry months, from November to May. Explosives are seldom used on Indian mines, but in cases where the rock is exceptionally hard, large fires are kindled against the face, upon which, when hot, water is thrown. This causes cracks to develop, into which iron wedges are driven, and in this way large masses of rock are detached. The tools used in the mines are of a primitive nature, and are usually made locally.

The mica as it comes from the mines is in blocks, which are theoretically short rhombic prisms, but are practically scarcely recognisable, as such, having a very rough and uneven contour. They have a very perfect cleavage, parallel to the base, and may be split into laminæ thinner than the thinnest tissue paper, in fact, it is possible sometimes to split the mica down so thin as to show colour like a soap bubble. The exterior portions of these blocks are opaque, brittle, and worthless, presumably from the penetration of water. A thick layer of plates has consequently to be removed from either face of the block before any mica of commercial value is reached, and the sheets split from the remainder are surrounded by a wide margin of worthless material which has to be trimmed away. But this does not represent all the losses in mica mining. Even when occurring in large blocks it is rendered valueless by one or more defects, such as colour, inclusions, cracks, ribbing or wedge formation. Colour is a defect only for certain purposes. Inclusions of quartz, garnet, tourmaline, and magnetite, are common, frequently decomposed felspar or china clay and other outside materials are washed in between the laminæ by rain and surface water, all of which defects seriously injure its value as clear mica, such as opticians would require. Often the mica is found to be ruled or cut into strips parallel to two opposite sides of the crystal; these strips fall apart as the mica is split, and are known in India as "Puttries." This, again, seriously decreases the value of the crystal. Sometimes the crystal is double ruled, dividing the plates into small diamond-shaped pieces; again, as mica is one of the softest of the minerals that make up the earth's crusts, and earth movements that take place will always tend to crush or distort the mica before the adjacent rocks; hence mica is often found badly ribbed or corrugated. Wedge-mica or cross-grained mica is mica that is uneven in thickness, with the laminæ running into one another so that it cannot be split; such mica is worthless, its only merit being that it usually makes a beautiful object when examined by polarised light.

By this time I hope that, if I have made myself sufficiently clear, you are beginning to understand why mica is so highly priced; it is not that it is a rare mineral like gold or platinum, but because of the uncertainties and risks in mining it, and the extraordinary amount of waste between the mica brought to the surface and the trimmed commercial plates. Even now, having got our good plate of mica out of the mine, the waste is not ended. It is of somewhat irregular shape, and must be further diminished by cutting to size, and thus about 50 per cent. of the good plate follows on to the scrap heap—finally, very often, the squared plates have to be cut into ovals or curved shapes before being finally used.

To sum up, it is estimated that the cut mica, as it reaches the market, is not more than one-thirtieth to one-hundredth part of the mica mined (nearly always the latter figure is nearer the proportion), and as a rule not more than one five-thousandth (1/5,000) to one ten thousandth (1/10,000) of the rock excavated.

Mica is principally mined in India, Canada, United States, Ceylon, and South Africa, and smaller quantities are received from all parts of the world; from Japan, Australia, Greenland, Brazil, Guatemala, Peru, Mexico, the Argentine, Siberia and other places too numerous to mention. Practically all the mica used in Europe, and a large part of that used in America—except the mica mined out there—comes to London, the headquarters of the trade.

From the mineralogical point of view, the "Micas" fall into three groups of species:—

1. The Mica group, which comprises the micas proper, including the commercial micas.
2. The Clintonite group, or brittle micas.
3. The Chlorite group.

All the above have characteristic micaceous structure; that is, they have highly perfect basal cleavage, and yield easily thin laminæ. They all belong to the monoclinic system of crystalline structure. All of them show on the basal section plane angles of 60° to 120°, marking the relative position of the chief zones of forms present, and giving them the appearance of hexagonal or rhombohedral symmetry, and they are all closely related amongst themselves in the angles of prominent forms. Chemically they are silicates of aluminium.

We are only concerned with the commercial micas to-night, which are here shown on this table, giving a typical chemical composition of each by Prof. Heddle, the hardness by Prof. Dana, and the specific gravity and optical data by Prof. Silvanus P. Thompson. In addition to these may be mentioned roscoelite, which is chemically similar to biotite, but contains 20 to 28 per cent. of vanadic acid, for which it is mined in California, vanadium being of value for making vanadium steel.

By some authorities muscovite is thought to be an altered biotite, the two are frequently found together in the same mines, sometimes crystallised together, a nucleus of biotite being surrounded by muscovite, so perfectly crystallised together that cleavage plates can be split off as though from one crystal. Again, other authorities class phlogopite, biotite, and lepidomelane

| Variety of Mica | Silica | Alumina | Iron Protoxide | Magnesia | Potash | Lithia | Fluorine | Water | Also traces of | Hardness | Specific Gravity | Axial Angle | Index of Refraction Air = 1 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Muscovite | 45·1 | 36·6 | | | 11·8 | | traces | 4·5 | | 2·2 to 2·5 | 2·7 to 3·9 | 55° to 75° | 1·56 to 1·61 |
| Phlogopite | 41·0 | 14·0 | 2·0 | 28·0 | 8·6 | | 2·57 | | | 2·5 to 3·0 | 2·72 | 7° to 18° | 1·56 to 1·60 |
| Biotite | 39·0 | 17·0 | 10·0 | 20·9 | 9·0 | | | | Titanic Acid | ditto | 2·5 to 3·0 | 0° or nearly | 1·54 to 1·57 |
| Lepidolite | 45·1 | 36·6 | | | 11·8 | 5 to 6 | traces | 4·5 | [illegible] | 2·5 to 4·0 | 2·8 | 76° to 86°<br>0° or nearly | 1·59 to 1·60 |

as varieties of one mica, according to the amount of iron in the chemical composition.

It is probable that all micas are biaxial, the so-called uniaxial micas always show some distortion of the coloured rings under the polariscope, and I believe also that monoclinic crystals are invariably biaxial, doubly refracting, and when coloured trichroic*—that is, they display three different colours when viewed by transmitted light in different directions—all of which characteristics are shown by the micas, more or less.

Lepidolite is sometimes given as a variety of muscovite, and so varied are the opinions that I have come across on the micas generally that it is evident that there is much to be discovered about them yet. They seem to form one of the most baffling of the minerals that the mineralogist has to deal with. To give only one instance of the confusion that exists, Prof. Dana, one of the best authorities on minerals, gives the axial angle of the lepidolite as large from 50° to 72°, yet all the pieces of lepidolite that I have ever examined show a very small angle, almost uniaxial, in fact.

The only mica of use to the optician to any extent is muscovite, although some of the dark biotites might be useful to protect the eyes from a glare, such as for the sight holes of bright sources of light.

One of the earliest uses of mica was for windows and lanterns, and for a long time, in fact, until quite recently, the mineral was called "muscovy glass" or "vitrum muscoviticum," possibly because the crystals were obtained from Russia; the Russians in turn receiving them overland from India, and probably also from one or two mines in Siberia.

It was, however, subsequently replaced by the cheaper artificial material, glass; but mica is still retained for this purpose where it would be subject to heavy shocks or violent vibrations—as, for example, in the conning towers of warships and submarines and in lanterns where there would be considerable risk attending breakage.

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*Ref. "Mineralogy," by Frank Rutley, pp. 24 and 33.

No artificial transparent substance has been devised to replace mica where high degrees or sudden changes of temperature take place.* It has, therefore, a wide and increasing use for anthracite, gas and oil stoves, transparent lamp chimneys, furnace sight holes, etc. Its transparency is little affected by repeated and alternate heating and cooling, and it is not attacked by gases and vapours. It is also used for screens in the laboratory and workshop for observing the processes in highly-heated furnaces without the observer suffering from the intense heat, since it does not transmit heat waves readily, differing from glass in this respect.†

This property is made use of in optical lanterns, where a plate of mica is often placed between the source of light and the condensers to protect the latter and the slides from the heat. I would suggest to opticians that this property is worth investigating further, as it might have many uses; for instance, would not goggles of a dark coloured transparent mica be better to protect the eyes when using the oxo-acetylene process of welding instead of the glass goggles which are used at present to shield the eyes from the glare and the intense heat? Mica has the additional advantage of not breaking and splintering as glass does under concussion. Mica goggles are used for motor driving, sand-blasting, and for many other purposes where they are likely to be struck by flying chips; it is also used for the sight holes of divers' helmets and smoke helmets. It is remarkably stiff in thin sheets, and an early use by the optician was for compass-cards, for which purpose it is still largely used, particularly for spirit and transparent compasses.*

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*Melting point about 1250° C.

†Taking the transparency of window glass to heat rays as 100, mica is about 60.

By far the largest use of mica to-day is, however, in the insulation of electrical machinery, being one of the best insulators known.† All attempts to produce artificially a substance which will combine the insulating and incombustible properties of mica have hitherto failed, and mica seems likely to hold its place for a long while to come without a rival in this field.

From the optician's point of view, one of its most interesting properties is the ease with which it can be split into very thin and perfectly even, clear plates, such as quarter-wave plates for polarisers. Those of you who attended the Optical Convention Exhibition last year will, no doubt, remember seeing the large quarter-wave plates of mica shown by Prof. Thompson. One of these was 19 inches in diameter, others were 10 inches and 12 inches square, and 20 by 10 inches, the largest size previous to this exhibition that were obtainable being only about 3 ins. square. That perfect quarter-wave plates can be obtained of these sizes shows how beautifully this crystal will split in expert hands, although I could hardly advise the working optician to attempt such large sizes.

Perhaps a few notes on the buying and handling of mica would not be out of place by way of conclusion.

The only mica that the optician is likely to require is perfectly clear, flawless sheets, and probably not one per cent. of the trimmed mica that comes into the market, even of the best grades, answers to this description, consequently suitable crystals command a high—and increasing—price. Mica can be obtained in a wide range of shades. No mica is absolutely free from colour, although one very rare variety of muscovite is nearly as clear and white as plate glass. This was discovered a few years ago, somewhere on the coast of Labrador. A small case of it was sent to England, but the locality of the mine has apparently been lost since then, and has not yet been re-discovered. The colour of the clear mica obtainable varies from very pale to very dark green and brown; in small sizes it is also obtainable in a pale lemon colour through orange to a deep ruby red. It is better for the optician unless he proposes to use mica extensively, to buy the plates he requires ready cut and split from the merchant. It is most important that the actual size wanted should not be exceeded, and where an oval or irregular shape is required a template should be sent with the enquiry, as often an odd shape will be considerably cheaper than its nearest squared size. Where practicable, the fullest details should be given to the merchant as to what the mica is to be used for, and what conditions it is likely to be subjected to; the merchant, having spent his life studying this mineral, is often able to make valuable suggestions. Large crystals of clear quality mica are very rare and difficult to obtain in sizes much above 6in. by 6in., although it is possible to obtain plates up to about 12in. by 18in., and a few even larger than that.

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*The specific gravity of mica is the same as aluminium, about 2.6.

† The break-down strength of mica is about 2,000 to 7,000 volts per mil (1/1,000th in.) of thickness, according to the quality tested.

For those opticians who are likely to require a fair amount of plates of varying sizes the best quality to buy for clearness and ease of working is the clear ruby mica, which is mined in Bengal. This is a muscovite of a clear pale brownish or reddish tint—a piece half-an-inch thick is a beautiful ruby red, hence its name, ruby mica. The pieces are trimmed to irregular more or less rounded shapes by having their rough edges cut away with a knife held on the slant—which trimming is very easy to split. It is obtainable in eight grades as follows:—

| | | | |
|---|---|---|---|
| "Special" | to cut rectangular plates of | 48 sq. ins. | and up. |
| "A.1." | ,, | 36 to 48 | ,, |
| "No. 1." | ,, | 24 to 36 | ,, |
| "No. 2." | ,, | 16 to 24 | ,, |
| "No. 3." | ,, | 10 to 16 | ,, |
| "No. 4." | ,, | 6 to 10 | ,, |
| "No. 5." | ,, | 4 to 6 | ,, |
| "No. 6." | ,, | 1 to 4 | ,, |

I have a few pounds here of the No. 3 grade to show the general appearance.

Mica is a very soft mineral, about as soft as selenite, and must be handled with the greatest care, as the slightest contact with any hard substance will scratch the surface, whilst if the edge is rubbed against anything the laminæ may commence to open and allow air to enter, causing air bubbles. Mica also readily shows finger marks on the surface, and consequently should be handled by the edge only—like a wet photographic plate. If it is necessary to dust the surface a piece of fine, well-washed chamois-leather should be used. In splitting the mica do not attempt to split thin plates off a thick slab one after the other, but try to keep on halving the thickness until it is thin enough for the purpose required, the knife used for splitting should be sharp on the *point only*, and must be quite blunt on the sides. As soon as there is sufficient opening between the laminæ a knife covered with chamois or velvet should be inserted, and the mica split apart carefully with this. By this means a plate is split open without scratching the surface of the mica. For further hints on the manipulation of mica the optician can hardly do better than read the chapter on mica-work in Lewis Wright's well-known book on "Light," wherein he goes very fully into the subject.

My authorities are numerous, including Prof.

Dana's well-known "Text Book of Mineralogy," Mr. Hugh S. de Schmid's recently published book on "Mica," and Prof. Silvanus P. Thompson, to whom more than to anyone else my best thanks are due for the never-failing kindness and courtesy with which he has answered my numerous questions concerning this mineral from time to time, and his interest and encouragement have been of the greatest help to me when studying this crystal.

# ON THE EVOLUTION OF EYEGLASSES AND SPECTACLES.

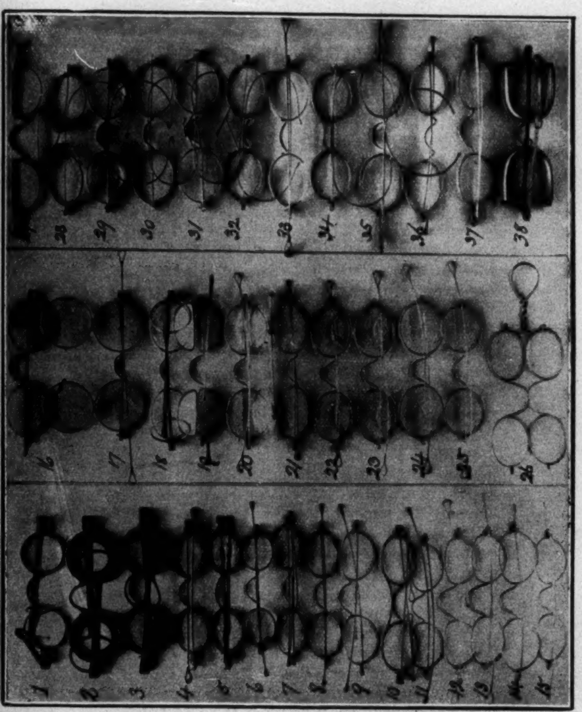
BY M. W. DUNSCOMBE.

*Paper read 13th March, 1913.*

THE earliest record of the usefulness of lenses to the eyes for reading does not at present appear to be earlier than A.D. 1317, although such a portrait of St. Paul writing his first epistle to the Romans is to be seen in the French "Bibliothèque Nationale." The illustrations of the Saints in the 14th, 15th, and 16th centuries, are not absent. Illustrations of Chinese reading eyeglasses are in the 17th century, and of those of France, the Netherlands, Germany, Italy and Spain. All the European eyeglasses up to the 18th century are forms similar to those in general use fifty years ago in England, as No. 146 with pivots and No. 91 with bows.

Samples in the Great Exhibition of 1851, of the Dutch eyeglasses with spring bow, A.D. 1600, and of the Chinese eyeglasses same date, were among those burnt at the Brussels International Exhibition of 1910. The only illustrations of supporting the eyeglasses with cords around the ears, are by Dr. Capivacio, Professor of Philosophy and Medicine, 1580, and the Chinese of the same period. In the publication by Mde. Alfred Heymann, 1911, "Lunettes et Lorgnettes de Jadis," illustrations first appear showing the style of English spectacles, A.D. 1700.

Accepting the early forms, as the usual form in comparatively small demand, the use of vision forward from A.D. 1800 made the demand for eyeglasses comfortable to wear; not every one had noses like the Duke of Wellington. Many variations to the early pivot patterns, Nos. 78, 92, 146, and 156, and bowsprings, Nos. 91 and 147, followed. No. 145, oval instead of round, was more elegant for ladies. The band forms,



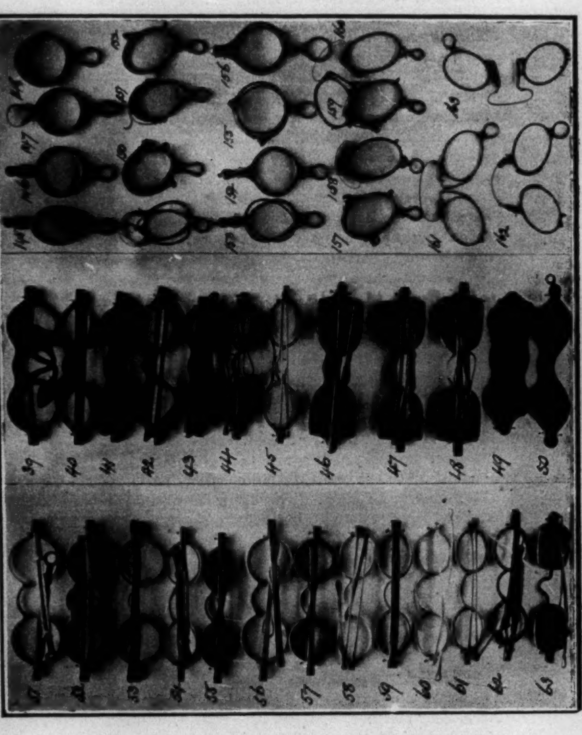
Nos. 148 and 152, were followed by No. 150, the helical patent of 1861, and the plaited spring of patent of 1863.

Patterns Nos. 153 and 154, with box spring patents 1850 and 1854, were very comfortable tortoiseshell frames, but heavy and often uncomfortable with gold frames (No. 76) both oval and round. Nos. 77 (gold) and 155 (shell) and similar steel frames with elastic gold or steel springs, were good for suitable noses. Nos. 90 and 95, with large lenses, fairly centre the eyes, when noses are thin. No. 84, with rigid folding bars, were convenient to hold in the hand and durable in wear. The springs with coiled springs Nos. 90, 93, 94, 95, 157 and 112, with aluminium frame, were successful A.D. 1862, but the "Prima Donna" registered in 1874 was more comfortable and successful until superseded by the springs Nos. 160, 161, and 162, because more certain to clip in this form.

No. 163 is a Dutch design for a comfortable combined spring and plaquet, rubber mount. Nos. 97, 98 and 100, sometimes known as "perfection," with combined springs and plaquets, and No. 99 have projections above or below the plaquets for suitably holding the nose. About 1888 the French pattern No. 123 was introduced —a clip with adjustable upright bar, for use with cylindrical lenses; then with No. 124 flexible bar. No. 123 did not please the English elegance. Curry and Paxton introduced No. 125 with round bars. The appearance of this was not neat enough, but soon afterwards the oval flat bar was made satisfactory, and followed the triple bar No. 126, patented in 1873 by Purdom and Boyd.

Various styles are Nos. 128 oxydised, 129 "Surus"; 130 by Makins; 131, with plaquets different lengths as required by unequal distance eyes; 132, 133 and 134, "Revluc"; 113, "Movillette"; 114, "Spectalette"; 115, 116, 117, 118, 119, 120, and 121, are all useful at times; also Archer's "Feather" patent of 1889, Sandhurst of 1898 and 1901, and the rigid clip patent of 1902.

The evolution of neat and rimless clips has been in progress for nearly twenty years. In the Great Exhibition of 1851, lenses with rimless folding mounts were introduced more from curiosity than utility. Pattern No. 135 in A.D. 1873, at a general price of half-a-crown each, proved a great success. It was soon followed by



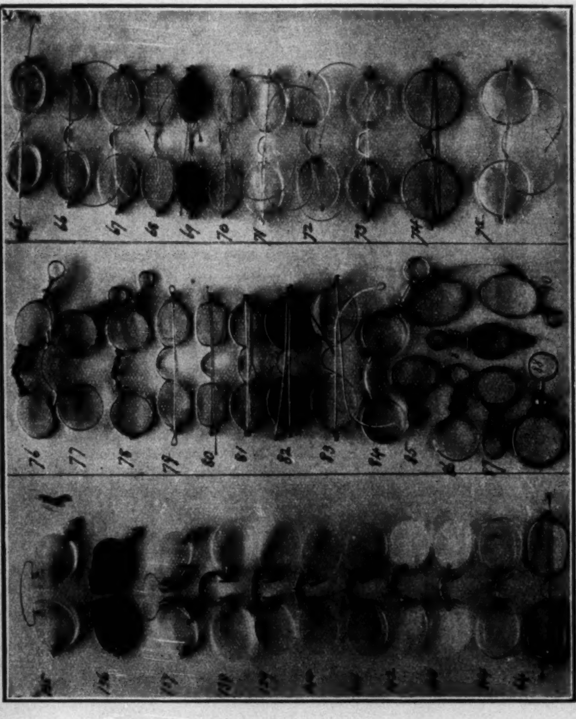
No. 137 with cork plaquets, and No. 136 with coloured flats, smoke and blue.

In 1895 the United States introduced an improved pattern; successive variations, Nos. 139, 140, 141, 142 and 143, have produced fittings to clip, to enable comfortable fittings to noses, among the best being No. 141, patented in 1904 by H. Newbold; and in the United States several plaquets or guards similar to No. 144, generally known as the "finger piece," a variation in this respect from the "Movillette" originally made in England in 1894.

The record of spectacles does not seem to show its existence before A.D. 1700, although some must have been. The earliest spectacle we have is No. 39 of tortoiseshell, about the time of Queen Anne, two of which among others, were burnt at the Brussels Exhibition in 1910; these were exhibited in 1851. Nos. 40, 41, 42, 43, 44, 45, 46, and 47 are various styles up to the middle of 1850. Nos. 44 and 45 have tortoiseshell fronts, silver turnpin sides, the former English, the latter French.

About 1836, some of the tortoiseshell frames were made for distance and near, the distance lenses vertical, the reading lenses set at an angle of forty-five degrees. No. 46 D eye shell front with shades and folding sides, gold joints to fronts and sides; No. 47 D eye shell fronts with shades, silver turnpin sides and silver joints to front; No. 48 with silver fronts, shades and turnpin sides.

Steel and silver spectacles were made for durability like the steel and iron cases of early times. No. 1 with folding sides and round bows; No. 2 with large bows for securing the sides to wigs; No. 3 with folding sides and concave glasses was dug from the ground at Bath after being used in the reign of George III. about 1750. The silver frames Nos. 51, 52, 53, 54, 55, 56, 57 and 59, are of wear about 1720 to 1820, some with sliding sides. Nos. 58 and 61 are lighter frames with turnpin sides, the former silver, the latter German silver. Nos. 62 and 63 are brass frames with sliding sides; No. 63 is stamped "Sweitzer," has side protectors, octagon glasses, evidently for American wear by Germans.



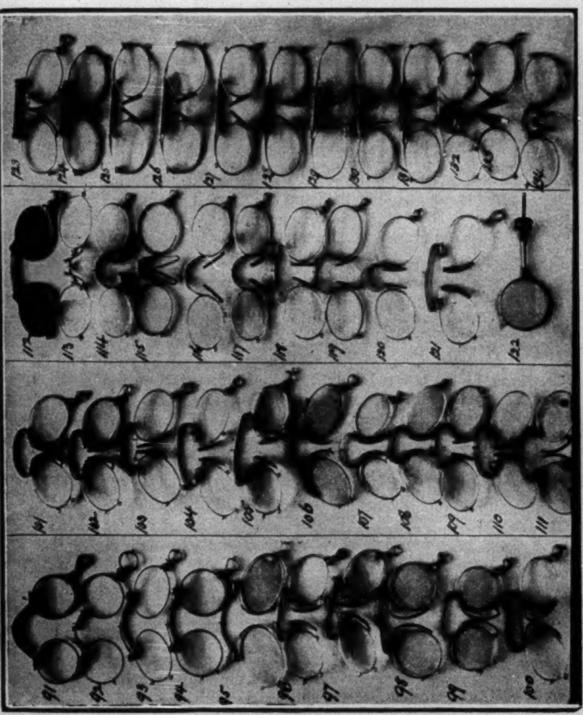
In the evolution of spectacles since the year

1834, improvements have constantly been on the move as to the frames as frames, and as to the proper fitting of the frame to the wearer and to the correction of lenses. No. 6 as introduced by Sir Isaac Newton is not an improvement; others will be mentioned as we go on. The improvements Nos. 8 and 9 in the frames, for a time were perfection, both made with solid fronts; when workmen have the skill, all is well; at present this style is not demanded. Nos. 11, 12, 13, and 14, as serviceable pantoscopic frames, and No. 15 as a very light frame, are all good.

The best quality steel frames in the past were London and Sheffield make; Birmingham and Wolverhampton now turn out thoroughly good work, but the style was not exactly the same. Nos. 33 and 34 are of the best, and among the best is No. 32 with Purdom and Boyd's patent bridges. Among the best light frames are Nos. 68, 69 and 70, light as thread and grooved lenses, of French make about A.D. 1850, No. 70 having the hooksides to fall horizontal and avoid strain on the fronts. At the present time the lightest and safest in use is No. 72, Hawes and Newbold's patents, 1907 and 1909. Nos. 71 and 73 are among the lightest frames, the joints 71, 72 and 73 have advantages in lightness of convex lenses that it is possible to use, but the screw-clamped joints to concave lenses are not so good in appearance and lightness as with grooved concave lenses.

The evolution of bi-focal spectacles since Franklin's half eyes in 1760 has made good progress, especially since when about 1850, solid bifocals were introduced. The introduction of No. 16 by Franklin was followed about the year 1820 with No. 17, using rising or lowering fronts by Kitchener, and then by No. 18 with side shades. No. 20 was made with the reading power placed pantoscopic like, with an angle of 30° to 45° and the upper part vertical with distant power. No. 21 has removable lenses for adjusting reading or distance power.

About twenty-five years ago No. 22 pattern with split segments became favoured; but the coarseness of the joins were not satisfactory in



neatness, and the evolution went on by stages to the "perfection" and the surface-cemented lenses. Perfection was reached by the "Dunscombe" invisible bi-focals, having inserted segments, and finally by the "fused" bi-focals with no risk of separating the segments.

No. 26 is a reversible form made in 1890, and No. 27 a front for use with spectacles to alter the power. No. 28, "Zephyr," relieves the nose from pressure and weight. Nos. 29, 30, 31, 32, and 34, are various hooksides; No. 74 with turnpin sides, and No. 75 with hooksides, both for shooting, billiards, lawn tennis, golf, etc. No. 38 D eyes, with stout pebbles as protection against gun shots. No. 64 with prisms. No. 122 to attach to brim of hat for reading or shooting. No. 79 shows a gold pantoscopic frame A.D. 1850. No. 80, octagon shape, gold American, about 1840; No. 18, American gold turnpin, about 1860; No. 82, bent preserver, gold turnpin sides, A.D. 1855; No. 83, with "Dunscombe" bi-focals, hooked gold frame, 1903.

No. 89 is an early double eyeglass, A.D. 1840. Nos. 85, 86, 87 and 88, show the patterns of single eyeglasses more than fifty years ago of frequent use, now used very rarely. Nos. 4 and 5 are samples of brass frames early in 1800, and No. 10, iron turnpin sides made in France, a little later date. No. 49 is the remains of a tortoiseshell hand case about 100 years old and a horn hand case about fifty years old.

Mrs. Grant, of Montreal, after the death of Dr. S. S. Grant, sent me a considerable collection of spectacles from Canada. Among them is No. 123, one of the clips with vertical bars; No. 35, a pair steel spectacles worn by "Beaupré," a French giant, with front about 6½ inches wide! No. 36, an unusual shape of frames in Canada; No. 64, large prisms, octagon eyes; No. 50, horn Hand case; No. 163, pair Dutch eyeglasses; and No. 63, brass spectacles, with shades; No. 73, Canadian frameless cemented split lenses.

No. 37 is a pair of pantoscopic frames, wide head, occasionally worn in Bristol.

# ON DIFFICULTIES MET WITH IN FILLING PRESCRIPTIONS FOR SPECTACLES.

### BY MR. WALTER GREEN.

*Paper read April 10th, 1913.*

IN this paper an attempt will be made to bring before your notice the more important difficulties met with in filling optical prescriptions for spectacles and eyeglasses, irrespective of the cause or causes from which they arise, and to indicate how far it is possible to meet the real difficulties and those due to the confusion of terms and systems in use.

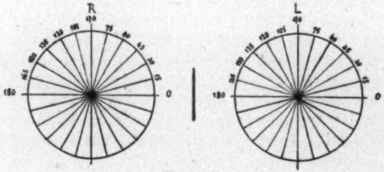


FIG. 1.

The question of the most suitable notation for cylindrical axes has been up for consideration time after time, before committees in this country, committees in other countries and international bodies, and we still await the acceptance of one universal notation; and those of us who are daily transcribing for workshop convenience other notations into the Standard Notation recommended by this Society await it perhaps with more eagerness than the rest. These numerous notations are inconvenient and complicate a matter which really ought to be perfectly straightforward; in the meantime, misunderstandings occur, and consequently delays and some mistakes.

There are four notations in general use. In the Standard Notation Angle recommended by this Society (fig. 1), the zero position is to the right, facing the patient. The radius vector rotates counter-clockwise, and thus turns through a positive angle. When writing prescriptions, in the absence of a diagram no indication is necessary other than the letters O.S. or S.N. following the number of degrees. No difficulty will arise in thus using it, except that perhaps, not having reached the remotest corners of the earth, a patient requiring a renewal of glasses may present the prescription to an optician to whom the letters O.S. or S.N. might convey a doubtful meaning.

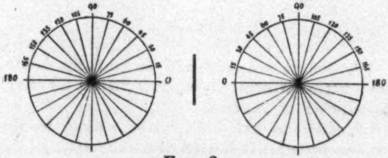


Fig. 2.

It would present a universal convenience if all prescription forms were headed with a suitable diagram showing the notation in use, and while not settling the vexed question of the most suitable notation for use, would render, at least, misunderstandings impossible.

The second notation to be mentioned is sometimes known as the Bi-nasal (fig. 2). For the right eye zero is to the nasal side, and consequently agrees with the Standard Notation; but in the left eye the zero line is also to the nasal side, and the line travels clockwise.

The third noted (fig. 3) is sometimes named the Bi-temporal, the zero being to the temple side in each eye. For the right eye the line moves in an opposite direction to the right eye of either the Standard Notation or the Bi-nasal, that is, with the hands of the clock. The left eye agrees with the Standard Notation and differs from the Bi-nasal.

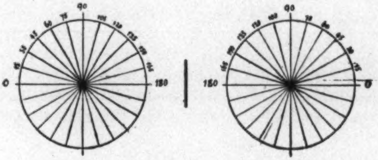
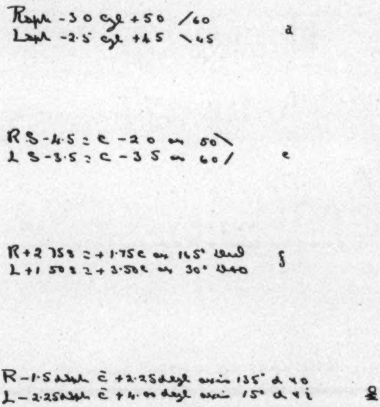


Fig. 3.

The fourth notation will be brought before your notice presently because it merits a special description of its own.

In order to avoid confusion owing to the various systems, it is a frequent practice, in the absence of a diagram, to indicate the direction of the cylindrical axes by means of a line (fig. 4, *d* and *e*), fixing also the number of degrees but this is not entirely satisfactory because the line, in many cases, does not approximate to its correct position, and consequently the axes may be set to a notation having its zero vertical. Another method free from ambiguity is to write the number of degrees and to indicate the positions of the axes by the letters D. and I. or D. and O. (*f* and *g* in fig. 4), meaning respectively "down and in" and "down and out." The axis in the former case would, of course, slope in towards the nose, in the latter case the axis would slope down and towards the temple.

It has been said that the fourth notation merits a special description. It merits a special description because in the absence of a diagram or a knowledge of its form it becomes impossible to interpret the various positions for the axes. Continental prescribers employ it, but with this important difference: the plus and minus signs do not appear. Illustration 5 gives copies of prescriptions written in this notation; *h* and *i* are interesting. 70° + and 60° – transcribed into Standard Notation are 20° and 150° respectively; it will be seen from this that the signs have no mathematical significance.



Rsph -3.0 cyl +5.0 /60
Lsph -2.5 cyl +4.5 \45
a

R S -4.5 : C -2.0 ax 50\
L S -3.5 : C -3.5 ax 60/
e

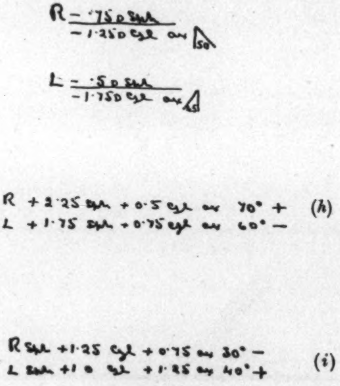
R +2.75 S = +1.75 C ax 165° [illegible]
L +1.50 S = +3.50 C ax 30° [illegible]
f

R -1.5 sph c̄ +2.25 cyl axis 135° d 40
L -2.25 sph c̄ +4.0 cyl axis 15° d 4 i
g

FIG. 4.

Turning to the diagram fig. 6, the zero position is vertical; for the right eye in the first quadrant the revolving line turns clockwise and the angle described is considered positive; in the second quadrant the line moves counterclockwise, and the angle is considered negative. For the left eye the numeration is the same, but care should be taken not to confuse the quadrants; the nasal for the right and temporal for the left are the positive quadrants; the temporal for the right and nasal for the left are the negative quadrants.

No difficulty would be experienced with prescriptions written in this notation provided a diagram always appeared, but prescriptions are frequently presented without this diagram. Especially is this so when formulæ are given on



R − ·75 D sph / − 1·25 D cyl ax 50

L − ·5 D sph / − 1·75 D cyl ax 45

R + 2·25 sph + 0·5 cyl ax 70° + (*h*)
L + 1·75 sph + 0·75 cyl ax 60° −

R sph + 1·25 cyl + 0·75 ax 30° − (*i*)
L sph + 1·0 cyl + 1·25 ax 40° +

FIG. 5.

hospital letters, not only in this notation but in others, and it is a hazardous undertaking to have to dispense prescriptions under such conditions.

The use of prisms in ophthalmology is restricted, and this is perhaps fortunate, because there are difficulties attending their use somewhat analogous to cylinder lenses. There are three notations in common use in this country, a fourth is occasionally met with, and others have been suggested from time to time. It is, therefore, insufficient to write "No. 2 prism," or whatever strength is needed, the sign for the prism diopter, the prism angle, degree of deviation or centrad should follow.

The various methods of numbering prisms give rise to some workshop difficulty. It is too much, under present commercial demands, to expect a lens grinder to have a scientific mind of ripe experience possessing a knowledge of all these systems, whether in practice or suggested, consequently in most cases prisms have to be transcribed into terms in keeping with the rest of his knowledge, a labour not free from possible error.

There is another matter in connection with prescriptions that may be incidentally mentioned here, that is prescriptions written in cipher. Such prescriptions cannot, of course, be deciphered in the absence of the key; they should be sent to the optician for whom they were intended.

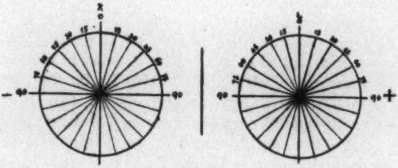


FIG. 6.

It is known that a positive lens decentered becomes equal to that same lens plus a prism with its base in the direction of decentration, but for a negative lens the base is in a direction opposite to that of the decentration. The power of the prism depends on that of the lens and the amount of decentration. This gives at once a ready means of introducing a prismatic element where possible, but it does not seem to be generally known how extremely limited these decentrations are.

A cx. one-diopter lens decentered 10 mms. inwards becomes in effect a one-diopter lens combined with a prism of 1△, base in, or as some prefer to write it, apex out; and to obtain the same prismatic effect in a half-diopter lens the decentration would need to be 20 mms., but both quantities are impracticable either for vertical or horizontal decentration. The maximum diameter of a stock spherical lens is 47 mms., and the long axis of the standard oval, O.S. No. 4 or American No. 1 eye, is 36.5 mms., the difference being 10.5 mms., but 1.5 mms. must be allowed for edging. This leaves 4.5 mms. for decentration in or out, and this figure will be reduced where larger eyes are used. It can also be shown that the maximum vertical decentration available is 9 mms.

These values apply to lenses of from ⅛ to 4 diopters, beyond this power the diameters become less, not that the larger diameters cannot be maintained within reasonable limits, but because diameters have to be chosen for stock lenses such as to ensure the normal finished lens a minimum weight; and it need scarcely be mentioned that a +7D cut from a 47 mm. disc would be considerably heavier than the same lens cut from a disc 40 mms. in diameter.

This system of stocking considerable quantities of uncut lenses renders modern service and prices possible. The question of weight, especially in cataract lenses, becomes at once important, hence the need of lenses in the uncut state of minimum diameter. In the case of spherocylinders the possible decentration figures obtain only when the axis of the cylinder is vertical or horizontal; this follows from the practice of grinding the axis of the cylinder surface from corner to corner—in any other position the available decentration will be less. It will be seen from the figures given that very few prismatic combinations can be derived by decentration; they almost always entail special grinding and a consequent increase in cost will follow. It is more economical to grind spherical prisms or sphero-cylinder prisms outright than to produce lenses of large diameter for purposes of decentration. These figures suggest also that sphero-cylinders of large diameter can only be cut from stock lenses when the axis is placed at or near the horizontal or vertical. When in any other positions, these lenses will be special products.

The present method of treating prescriptions in which the powers of the two lenses differ considerably cannot be described as altogether satisfactory. The lenses must be equal in weight, otherwise the frame will have a tendency to tilt in the direction of the heavier lens, and should be—for comfort—as light as possible, but this latter condition is not generally fulfilled. The usual practice is to increase the weight of the lens of zero or lower power until it becomes equal to the heavier, thus the weight as a whole is much increased, and this increase becomes particularly noticeable where lenses of large size have been chosen. Then again, the edge thickness of the weaker lens becomes more evident in the direction of the long axis, and this cannot be avoided.

In all these prescriptions the heavier lens could with advantage be operated upon, and its weight become considerably reduced without impairing in any way its optical properties. Indeed, in some cases where high power equiconvex lenses are prescribed the modifications about to be suggested would be advantageous.

Consider as an example the prescription:—

R.E. +0, L.E. +10 D sph.

The left lens ground +11.25 and −1.25 knife edge, with a diameter of 26 mms., cemented to a plano peris support would effect a considerable saving in weight to the obvious comfort of the patient. The smaller field due to this lens cannot be urged against its adoption, because actually the useful field becomes slightly extended. Moreover, large diameter lenses, especially of the equi-convex type, are particularly bad, and serve no useful visual purpose. The trouble that may arise from a want of permanency of the cement is slight, and a matter of a few pence only for renewal. Further, such lenses are now made, and have been made for some long time, in solid form, and such might be chosen when cost is of little importance. When sph. cyls. are required the only modification necessary is in the support, the plano lens being replaced with a plano cylinder, and of minus power for choice.

The treatment of deep concave lenses for a reduction in edge thickness, and consequently in weight differs somewhat from the foregoing, and cemented lenses are not used. Considering an equi-concave lens, each face may be flattened, leaving a circular field of 26 mms. diameter for a size O.S. 4 or American 1; this, of course, may be increased when larger lenses are chosen; instead of grinding the peripheral portions of each face flat they may be ground convex, the final result being the same except that the edge thickness in the latter case will be slightly less. These lenticular lenses, as they are sometimes called, can be produced with an oval field the concave spherical curve then being ground on to a convex surface of a high power plano cylinder. For a high power plano concave lens or sphero-cylinder, the operation, of course, must be confined to one surface. Such are the methods available for the production of high power lenses of minimum weight; they are not put into practice unless special instructions be given.

Coloured lenses, as usually manufactured, cannot be said to be articles of efficiency or beauty. Consider the case of moderate and high power convex lenses. Such lenses are specified to have a distinct tint, *i.e.*, transmitting a certain quantity of the incident white light. It will be seen on consideration that the colour in these lenses cannot, without having recourse to special devices, be arranged evenly throughout the lens; it will vary at almost every zone. This defect in concave lenses becomes even more serious if a tint of 1, *i.e.*, transmitting 80% of incident white light be arranged at the centre; then a few mms. on either side of the axis the transmission is likely to be of the order of about 60%; beyond this the defect becomes more serious until at the margin the transmission will be very small indeed. But this defect can be remedied by the use of a thin, flat lens tinted to the required degree cemented on to the white plano convex or concave lens. The distribution of colour will, under these circumstances, be equal throughout the entire lens, and, in addition, the appearance of the lens becomes vastly improved.

This method serves for almost any lens, irrespective of its form. For sphero-cylinders the only change would be in the coloured wafer. This would have to be a plano-cylinder and would be cemented on to a white plano-convex or concave lens. Some combinations present difficulties, such as sphero-cylinders, in which the powers are more or less equal, but if the lower power be taken as the coloured member they will still yield a finished lens superior in this special direction to a combination ground from a coloured blank. If absolute coloration over the whole lens is insisted on, then it would be possible to cement a coloured wafer between the white plano-spherical and plano-cylindrical, and the end thus obtained.

If expense is of little importance, and time can be given, most if not all coloured lenses can be supplied with the tinted wafer fused on to the white support. The finished article is one of great beauty and efficiency. Messrs. Kryptok have been good enough to fuse some specimens, and they are on the table for examination at the close of this paper.

Amber-coloured glass is especially difficult to fuse on to white glass; indeed, it may be said that at present the production of these fused coloured blanks is a matter of some uncertainty, and their cost from four to five times that of the cemented lens already described. For tropical climates the fused lenses should be chosen when possible in preference to the cemented type, because in this latter case some trouble may arise from the cement.

The question of eye shapes needs consideration; they are of importance to the patient both from a cosmetic point of view and that of utility. The recognised shapes of eyes defined by this Society are round, oval, long oval, round oval, pantoscopic, half oval, and with the exception of the round oval eye shape, they give rise to no difficulty.

This round oval eye shape is in common use and enjoys the distinction of four names, in addition to letters and numbers. The various names given to this particular shape are deep panto, round panto, panto round oval, and it is further defined by others by means of a number or a letter, or a letter and a number. This multitude of names must, at some time or another, lead to difficulties, and something should be done to definitely settle such a small matter. The actual sizes cannot be offered as an objection because it is just as easy to order 0 eye round oval, or whatever size may be required, as any other shape in the series.

The round oval, as will be seen from the drawing, fig. 7, is derived from the circle and oval —oval in its upper, and circular in its lower form—half and half so to speak, *i.e.*, round and oval, and if we agree to drop the conjunction, then round oval.

There remains for consideration one other eye shape, the short oval. The meaning is as self-evident as that of the long oval. It is a type so frequently called for that it deserves a place amongst the other accepted eye shapes. Moreover, it is frequently confounded with the round oval, and its introduction to that place of honour would do much towards lessening the present confusion in terms. It is generally the dispensing optician who, through a confusion in terms, has to suffer the loss of what may be a costly pair of lenses, and not the least important is the fact that patients are kept waiting for lenses of which they may be in urgent need.

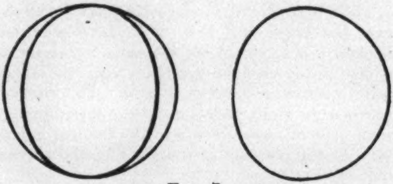


FIG. 7.

The length of the front of a spectacle frame, according to the methods of measurement recommended by this Society, is the distance from centre to centre of the pins. This front length is equal to the effective front length only when the sides are at right angles to the plane of the eye-wires. For any other position of the sides other points must be chosen from which to take this measurement.

In all that follows it is understood that stock articles are being considered, and how far it is possible to modify such to fulfil the exacting requirements of modern practice. There will be intervals to bridge, especially in the matter of front length or facial width, which is in the end the same thing; and when a clear understanding is established on such matters, one difficulty, and it is a real difficulty, will have disappeared.

If it were possible to specially fashion every spectacle and eyeglass there would be no need of these remarks, but such a course for economical reasons is not possible.

Then, again, if we narrow all difficulties down to G.F. spectacles, it will be found that special construction cannot be applied to these frames, not at least to the joints, because they cannot be fashioned by hand, and other things being equal, it is on this particular part of a frame that most attention would have to be bestowed in order to fulfil the many slight variations in front length.

The length of a G.F. spectacle front, standard oval No. 1 eye on a P.D. of 60 mms. with the normal 5 mms. joint, is 105 mms.; this facial width is increased by 8 mms., when the larger joint measuring 9 mms. is used. The interval between the two is large, and in order to bridge that interval some modification becomes necessary in the position of the sides in the former case.

It will be seen from fig. 8 that two positions for the sides have been depicted, and when in the position J C the effective front length becomes greater than the real front width measured from pin to pin. It is proposed to measure the effective facial width from points on the sides 25 mms. from the pins, because it is approximately at that position where the sides would cut into the temples in the case of a restricted front length. This increase will depend upon the angular value of $\phi$. Calling the increase T C, its length is given by J T tan $\phi$. When $\phi$ is 5° the total increase is 4 mms., and for a value of 10° is 8 mms.

If larger eye sizes be chosen they can be made by preserving their normal shape to increase the front width, but the increase made in this way is often disappointing. Likewise by reducing the eye size a small reduction is obtained, but this is rarely wanted; the difficulties lie in the other direction. It will have been noticed that for No. 1 eye the difference between the front width and centre is 45 mms., a quantity in general slightly too little. This can be increased by 1.5 mms. with No. 0 eye and 3.5 mms. by using No. 00 eye, thus by combining the two things the most difficult case can be met satisfactorily. If an increase in eye size can be made to extend the frame front it is also true that short oval and round oval shapes add complications, a matter that is constantly being overlooked.

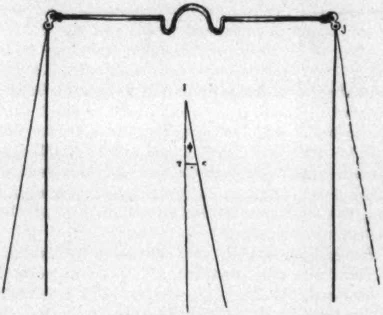


Fig. 8.

On centres of 60 mms. using the round oval, size 4, the front length will be 102.5 mms., that is 2.5 mms. less than that given by the oval shape, and this quantity is constant for reduction on all lens sizes. The employment of this shape for a big difference in facial width leads inevitably to the selection of the long joints, and they cannot be considered, except in some few cases, to add either to the charms of the patient or to the neatness of front considered alone. This method is adopted for increasing the facial width of gold, white metal and steel frames, but such modifications cannot be pushed too far. There are certain peculiar head shapes which limit its effectiveness by dragging the sides when in use on to the temples; and for heads which rapidly widen towards the ear the effect on the appearance may be a matter for attention. But in all cases this modification in side position can be made to contribute something towards overcoming a difficulty and reducing what, in some cases, would be a length of joint alike startling to the patient and fitter.

The centres of a spectacle frame together with the base measurement of the bridge will limit the eye size, and it may be stated at once that, considering the normal oval eye shape for centres of 50 mms., and less, if the bridge is to have a free position the eye size should not exceed the No. 2 American. This size may be increased to No. 1 if the round oval or short oval shape be chosen, but only then for centres not less than 50 mms. and a moderately small base measurement. It may be necessary for centres of 47 mms. down to 44 mms. to use the round eye, the diameter, of course, depending on the bridge measurement.

Something can be said about these various dimensions with certainty. Using a normal American oval No. 1 on centres of 53 mms. the separation between each eye-wire will be 15 mms. In other words, the maximum base width cannot exceed that amount unless both the shanks of the bridge and some portion of the body of the bridge come within the lens field. The depth of the bridge is another factor which will influence the spread. Any depth can be given to the bridge, but the greater the depth the smaller will be the spread.

It has been mentioned that for base widths greater than 15 mms. for this particular size, the shanks and bridge body will appear within the field of the lens, that means a sharp return of the shanks, and such sharp returns, especially in white metal, some gold-filled and even gold frames, cannot be looked upon without some misgiving. With steel it is different, because its properties are different from the alloys mentioned.

It may be said that for a bridge to possess a normal appearance the major axis, or the diameter of the lens, plus 1 mm. for the eye-wire, plus the base measurement, should not exceed the centre to centre distance, otherwise the bridge must present a cramped appearance and become liable to easy fracture.

The methods in use of measuring astigmatic eyeglasses and eyeglasses of the semi-rigid type need attention. It has been recommended by this Society that it is desirable to record the inclination and distance apart of plaquets. With straight plaquets the measurements to be taken at the top and bottom of the bearing surfaces; with curved plaquets at the top, centre and bottom; with rocking plaquets at the centre only, the plaquets being parallel, and these measurements should be taken while the frame is at rest and not while in use. Some workers give the distance between the eyes, others measure from the centres of the plaquet pins in front of the eye-wires, and in some cases the measurement is given from the centres of the plaquet pins behind, but these latter measurements are open to several objections.

Consider the two measurements taken from pin to pin (fig. 9), they differ to the extent of from 2 to 5 mms., depending on the amount of angling given to the plaquets about a vertical axis. Neither take into consideration the type or thickness of the plaquets, thus giving rise to a further error of anything up to 3 mms. The measurement taken from eye-wire to eye-wire is open to objections no less serious because this also takes into no account the thickness of the plaquets or variations in position.

No such objections can be made against the method recommended by this Society, because in measuring from bearing surface to bearing surface such variations are neglected. It is important to note that the measurements are to be taken when the plaquets are at rest and not while in use.

This plaquet space, as it may be called, will

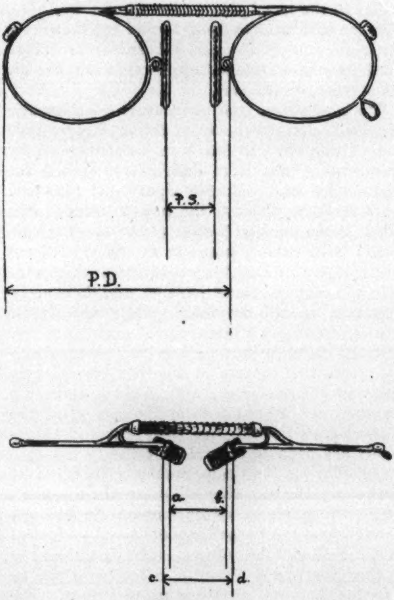


FIG. 9.

in general be from 2 to 3 mms. less than when on the patient's nose, to supply the required spring tension, and a corresponding reduction in the P.D. must also be made to harmonise the final measurements. It can be shown from the drawing that in addition to the eye size and inclination of the plaquets with the plane of the eye-wires only two other measurements should be recorded—the P.D. and plaquet space. To add further details serves only to overburden a specification, and gives rise to difficulties.

An astigmatic eyeglass, 1 eye, with centres 60 mms., and normal rocking cork-faced plaquets, will have a plaquet space of 16 mms. If a plaquet space of 12 mms. is required there remains to be fixed two extension pieces or spurs, each measuring 2 mms. to fulfil the conditions, and it is in this way that astigmatic measurements are carried out, but with ease and certainty only when these details appear alone. The precise shape to be given to a plaquet is another matter, and does not affect the argument. It may be even necessary to send a cast of the nose in order to have the plaquets correctly shaped, that again, except in abnormal cases, offers no difficulty.

Any set of measurements for a spectacle for distance should be modified to render it satisfactory for reading, but precisely what the alteration and the extent of these alterations should be is not generally understood. When the visual lines are directed to a near object they suffer convergence and depression, involving four modifications in the original specification. The centres of the lenses must be moved inwards and downwards and the front must be angled about a horizontal axis, this angle being added to the angle of inclination of the bridge. All that can guide the dispensing optician when asked to make such alterations are certain well-known facts regarding the depression angle, the rest he must deduce from whatever established laws are likely to render him the most useful aid.

It can easily be shown that the shift in position of the visual lines is not constant for any two base line measurements, in other words the inward decentration will vary with the P.D. The greater the P.D. the greater the convergence. Fig. 10, B.L. represents the base line, in this case 70 mms., BB′ LL′ the visual lines when directed to a distant object, BM LM the same lines when converged to an object 250 mms. away, $\theta$ the angle of convergence, *a f* the anterior focal plane taken as 27 mms. from B, the rotation centre. The length of *a f* is given by B a tan $\theta$, and this quantity will equal the convergence.

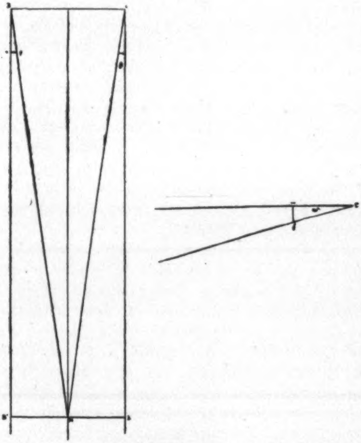


FIG. 10.

$$\tan \theta = B'M/BB' = 35/250 = .14,$$

whence *a f* equals 3.78 for each eye, or a total of 7.56 mms. For a base line of 50 mms., *a f* is equal to 2.7 mms. for each eye, or a total of

5.4 mms. This is roughly one-ninth of the whole.

For a reading distance of 33 cms. these quantities will be less, for a P.D. of 70 mms. the inward decentration will be 5.74 and 4.1 for a P.D. of 50 mms., roughly one-twelfth of the total.

If the angle of depression is taken as 10° the downward decentration equals 4.76 mms. for $af = ca \tan \delta$. When $\delta = 15^\circ$, $af$ becomes 7.23 mms.

The angle of depression, according to Dr. Percival, is from 13° to 14°. Taking the former figure the downward decentration should be 6.3 mms. The angle given to the front of the spectacle frame will equal the angle of depression whatever its value; this in order that the visual lines shall coincide with the optic axes of the lenses.

Although the exact numbers more or less are given in the columns marked inward decentration, it is impossible to carry such refinements into practice; the nearest working number should be taken. For convex lenses the decentration should not be less, they may have to be greater; for concave they should not be greater; they may have to be less.

The bridge measurements of reversible spectacles are extremely limited, and lead to many impossible specifications. It should be carefully noted that straight side reversible spectacles can be fitted with only two serviceable bridges, the W or the X bridges. The projection, spread and depth of arch in the W bridge can be of any desired amount within practical limits, but the height must be level, otherwise, when turned over to give a change in lenses, the bridge will be as much below centre as it was above before reversal. With the X bridge the spread depends largely on the P.D. and eye size; it can have no definite measurement. What has been said about the height of a W bridge applies equally well to the X. The W bridge can be made to conform to the outward or inward decentration by a change in lens position, whereas in the X bridge this change must be made by decentering the lens.

Reversible curl sides can be fitted with an X, K arch or crank bridge, the latter being the most suitable because the spread may be varied at will. In the other three types this will depend on the eye size and P.D. The K arch and crank can be given any height, but the projection must be 0, otherwise during reversal the plus projection becomes minus or *vice versâ*. A fixed W bridge cannot be used satisfactorily for cable side reversible spectacles. Fig. 11 perhaps shows why this cannot be more eloquently than words; it will be observed from the figure that during reversal the bridge would have no position on the nose.

Eyeglasses and their limitations for assymmetrical cases can only be briefly dealt with in this paper; the subject is a large one, and really needs separate treatment. Fortunately some of these irregular cranial developments do not affect eyeglasses, *e.g.*, difference in ear level and in ear position from back to front. Briefly, then, one eye may be placed further from the median plane than the other. There may be a difference in eye level; one eye may protrude abnormally proptosis or exophthalmos.

Considering astigmatic eyeglasses, rimmed and rimless, only one condition can be met, that of difference in eye position with reference to the median plane. To compensate for the other the one lens must be moved bodily upwards, forwards or inwards, and the eyeglass is unsuitable for such modifications. True, the lens could be decentered up or down for the non-level condition, but this is only suitable for small differences. For large differences of from 4 mms. upwards, not to shift the position of the lens is simply to call attention to the defect. The semi-rigid clips, both rimmed and rimless, can be made to fulfil all conditions, and should be chosen when eyeglasses are demanded. By operating on one or both studs the lens can be put into almost any position with very little structural alteration. With rimless the position of the hole may be made to contribute something towards the difference in eye level; for the rest the length of stud in the horizontal direction varied at will and an off-set piece of varying length made to meet the condition of proptosis.

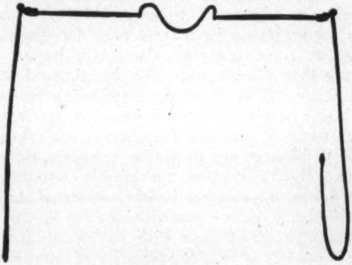


FIG. 11.

The finger clip is very difficult to modify satisfactorily for any one of these conditions. The shank length can be increased to meet unequal pupil distance. The plaquets ought not to be operated upon to compensate for this defect; they should be kept symmetrical about the arch of the bridge, otherwise the bridge will be displaced in addition to the lens, and the neat effect of the bridge fitting spoilt.

The difference in eye level can only be compensated for in the rimless type by drilling the lens above or below centre; this modification cannot be carried out with any degree of satisfaction in the rimmed finger clip. When one eye protrudes in front of the other a different form of eyeglass should be chosen, if eyeglasses are insisted upon.

A few words may be said about the finger clip for normal cases. This type of eyeglass has revolutionised modern eyeglass ware, and like many other recent types its limitations do not seem to be generally understood. Owing to peculiarities in their construction very little can be done to vary the centres for any given number. These eyeglasses are numbered according to their centres, height and projection of bridge considered with reference to some definite eye size, and it follows that the only possible variation in centres must be obtained by either altering the eye size or its shape. It is true that when a reduction in centres is required, and it is generally a reduction that is needed, a smaller size may be selected, *i.e.*, a lower number, and the plaquet space increased until it becomes equal to the reference fitting given. But this gives no greater reduction than 1.5 mms., and yet it is a matter of daily experience to receive prescriptions for this particular type of eyeglass with centres on a fitting hopelessly beyond the capacity of the manufacturer or the dispensing optician to supply. These eyeglasses, except for small fittings and lenses, are unsuitable for pupillary distances of less than 56 mms., otherwise the measurements must be obtained by an inward decentration of the lenses.

## Table of Decentration for Reading.

READING DISTANCE, 250 MMS.

| P.D. | Inward Decentration. |
|---|---|
| 50 | 5.4 mms. |
| 52 | 5.6 „ |
| 54 | 5.8 „ |
| 56 | 6.0 „ |
| 58 | 6.2 „ |
| 60 | 6.4 „ |

READING DISTANCE, 250 MMS.

| P.D. | Inward Decentration. |
|---|---|
| 62 | 6.7 mms. |
| 64 | 7.0 „ |
| 66 | 7.2 „ |
| 68 | 7.4 „ |
| 70 | 7.6 „ |

READING DISTANCE, 330 MMS.

| P.D. | Inward Decentration. |
|---|---|
| 50 | 4.0 mms. |
| 52 | 4.2 „ |
| 54 | 4.4 „ |
| 56 | 4.5 „ |
| 58 | 4.7 „ |
| 60 | 4.9 „ |
| 62 | 5.0 „ |
| 64 | 5.2 „ |
| 66 | 5.4 „ |
| 68 | 5.5 „ |
| 70 | 5.7 „ |

The CHAIRMAN said he thought they would agree that Mr. Green, who was the visiting lecturer at the Northampton Institute, had fully justified his reputation by the lecture on filling optical prescriptions. Of all the subjects presented, and they had a very wide range, he did not think they could have had one that appealed to such a wide section of the Society as a paper on the fitting of eyeglasses and spectacles. Mr. Green had given them a paper pointing out the many difficulties that were met with in filling prescriptions; and from his general remarks he took it that if there were more of a standard followed out by opticians there would be very little of this trouble. From his own point of view, he thought they had to look at it from two ways, very broad ways. If they had a very interchangeable standard, very possibly some of the gentlemen there might not be so prosperous as they were. They would have opticians at every tube station fitting people automatically. There was one point upon which he would like to see absolute universality, if he might coin such a word, and that was the question of notation. For the life of him, he could not see why there should be any diversity of opinion in the matter of notation. They themselves had adopted a standard notation absolutely, and would not dream of recognising any other. He had always considered it quite sufficient; and he thought Mr. Green would perhaps agree with him that the standard angle of notation was the right one. Mr. Green had raised a doubt which he did not quite follow, as to there being any uncertainty when the line was given. He should have thought that the fact of giving the line and the direction of the axes would have removed all doubt as to the direction.

He (the Chairman) knew that quite recently, within the last two months, not very far from where they were meeting, a well-known optician had to pay quite serious compensation to a customer because of a mistake in carrying out a prescription due to misunderstanding the notation. The prescription, he understood, came in from an oculist who adopted one of these solo notations, as he (the Chairman) called them, individual to himself. Fortunately for himself, the oculist had given the line of direction; but the optician, unfortunately for him, had transcribed the prescription on to a working prescription, and forgot to give the line of direction, with the result that the two lines of axes came out very differently. Mr. Green had pointed out many difficulties, but he had also given many valuable hints how to overcome them. He (the Chairman) thought Mr. Green's remarks as to unequal lenses were very valuable, and might be followed out; especially also the colouring of lenses. In some cases, it might be an advantage, where the edges of the lenses caused some aberration, to have a deeper colour in the margin, but probably the opposite would apply in bi-convex lenses.

Mr. Adams said he should like to second everything Mr. Sutcliffe had said with regard to denoting axes. That, he thought, was one of the chief difficulties they, as dispensing opticians, had to meet, and he thought Mr. Green was well within the mark when he said there were four ways. He (Mr. Adams) should have thought there were many more than four ways. He came across many more than four ways of denoting the axes, but he was glad to say, after twenty years, he had never come across a plus and minus. That must be extremely vague, and he wanted to understand it before he left. He had had rather an amusing experience some time ago. He once persuaded a London ophthalmic surgeon to adopt the notation which his Society had standarised, and he (the surgeon) told him, whenever he went to see him, that he had cursed him ever since. Another question had come up only that week. This was in the case of an ophthalmist who used the bi-temple notation. The 0 was on the outside. He had no diagram, but always drew the line. "On one occasion he drew the right eye, which is always the chief difficulty, in the standard notation. He had never done that before, and I went to see him. He said, 'Well, the only safe thing is to draw the line.' I said: 'You did, but you drew it differently this time.' 'Well,' he said, 'I only hope it is right.'" With regard to deep concaves (chamfering them down), I thought most opticians would like to do that a great deal more often than they did; but one of their chief difficulties—he spoke from the retailers' point of view—was in dealing with the customer; and while the optician would like many times to reduce lenses, the customer very much objected, and it was very difficult to persuade him that they were not any more disfiguring than a deep and thick edge. He should like to hear more of the + and − notation, and would be glad if Mr. Green would kindly draw it for them. With regard to reversible spectacles, they could be made with W bridges if they made the side to turn. He expected Mr. Green knew that well enough, but had overlooked it. It was also possible to make W bridges reverse. He believed it was a patent, but it was done in many cases. He wished to say that he had been very much interested in the paper, because he had had a very long experience, and he had met practically all the difficulties mentioned, and had had to overcome them. Of course, he did not wish to boast, but he had had facilities for overcoming the difficulties which every optician had not, in having a staff of workmen. He was greatly indebted to Mr. Green for a deeply interesting paper.

Mr. H. F. Purser said he hoped to hear many dispensing opticians speak on the subject. As a wholesaler, he very frequently came across the difficulties mentioned by Mr. Green, he thought almost as frequently now as before the Society introduced its standard measurements of parts of spectacle frames; and he thought this was possibly due to the fact that they omitted to publish those diagrams showing the standards. Although the standards were printed, no diagrams were given, and the matter did not therefore appeal so readily to the mind. He had met many opticians who had already forgotten that there were such standards. Even their friend, Mr. Adams, was apparently not aware of the existence of the short oval among the Optical Society's standards; and he (the speaker) was sure, if they could persuade the Society to publish the diagrams with the sizes and shapes of bridges, eyes, etc., they would go very far towards making the standards so popular that these difficulties would be got rid of entirely. As most of them were probably aware, there was still a great deal of misunderstanding in reference to the term "round-oval." There was continuous trouble with regard to the question of plaquet measurements. As far as his experience went, the majority of opticians used measurements other than the distance between the plaquet bearing itself. Many used the measurement from the pin when dealing with a movable plaquet, such as an astigmatic plaquet, and others the distance between the eyewires. As Mr. Green had pointed out, neither measurements stood—they varied considerably in different kinds of plaquets, and they were exceedingly unsatisfactory and untrustworthy as measurements for fitting. Mr. Green's paper had been a very interesting one, and would add very considerably to the interest which the trade ought to take in the standards of spectacle fitting parts.

Mr. Adams said there was one difficulty Mr. Green did not mention, namely, that met with in dealing with foreign prescriptions. In reading the axes of the eyes, they wrote their prescriptions as the patient looked through the spectacles, not as we did in England, looking at the spectacles on the patient's face.

Mr. H. F. Purser said he thought Mr. Green suggested that manufacturing difficulties would probably prevent the production of sizes and shapes of eyes other than the recognised patterns. It often occurred to him, in looking at individuals who were wearing eyeglasses and spectacles, especially since the introduction of frameless glasses, that a considerable improvement might be made by making the eyes contrary to the usually recognised shape, and more in keeping with the shape or contour of the individual's features. The same thing was true as regards shape of frames. A straight bar placed upon a face of delicately curved features, also a round spring frame on a person with straight eyebrows, often called attention to peculiarities of feature which might otherwise be detracted from. Some years ago, he had constructed an eyeglass with pear-shaped eyes, which he thought would suit a great many persons much better than the ordinary oval eye. He would place one on the table for inspection, and he thought they would agree it had a much better appearance on some persons than the ordinary oval eye.

**Mr. H. A. Hughes** said the matter that had specially interested him was the reference to tint, but Mr. Green had not much to say about the difficulties they had in dealing with tint. He found the difficulty especially in the lighter tints. There seemed to be no recognised standard.

**Mr. Green** said he had mentioned in his paper that there was a possible source of error when indicating the direction of the axis by means of a line fixing the number of degrees. He had met with that difficulty in practice. People who drew these lines frequently drew them in a careless manner, and he had seen axes which were intended to represent 30° from the horizontal drawn 30° from the vertical, and he thought the dispensing optician was justified in setting the axis according to the notation having its zero vertical. If the lines approximated to their true positions there could be no difficulty whatever. He was afraid he could not tell them why there were so many notations in use, but he could tell them what one authority had to say about such a matter, and he wanted to read to them what Dr. Percival had said regarding the notation of cylindrical lenses.

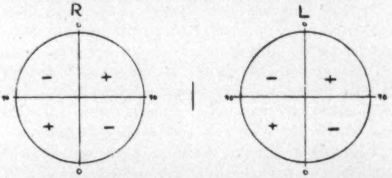
"This is still a vexed question; all, however, are unanimous on one point, 'that the meridians of astigmatism should be measured and represented as the observer looks at the patient.' With regard to the angle notation, there is almost every possible diversity of opinion. Some, paying attention to the undoubted bilateral symmetry of the eyes towards the middle line, consider that the nasal extremities of the horizontal line of the trial frame should be the zero for each eye, and that the angles should be measured from this point by a radius vector rotating through an angle of 180° to the temple. (This was the method submitted by the Commission at the International Congress held at Naples, 1909.) Here again, there is room for a difference of opinion which is a new source of trouble, some rotating the vector upwards and some downwards. Consequently, it is necessary when prescribing for patients to indicate the angle by a line (oblique when needed) to show which method is adopted. It is exceedingly difficult for the printer to indicate these axes by sloping lines, so that it is very inconvenient for printed reports of cases."

If that was the only reason they could offer for choosing a notation which involved two diagrams, he thought it was a very poor one.

"In contra-distinction to these ambiguities is the Standard Notation, approved by the Optical Society in 1904," already described, and one which Dr. Percival adopts in his book.

Regarding the + and – notation, it was peculiar to a certain district, and that was rather fortunate. Generally, when such prescriptions were met with a printed diagram appeared, and except for a decided practical inconvenience they offered no real difficulty. But prescriptions were frequently written on hospital letters with nothing to serve as a guide in the way of a diagram, and in very few instances are the axes indicated in any way except by a number. It was in such cases that great uncertainty existed regarding the true direction of the cylinder axis, and particularly was this so when the + and – signs were used contrary to custom. Then again, mistakes occurred in transcribing this notation into a suitable working notation for those who were not acquainted with this complicated system. As requested, he would draw the diagram of this notation again with the quadrants marked according to the signs allotted to them (see figure).

Regarding foreign prescriptions, he had not met with the difficulty Mr. Adams had pointed out, but his experience with these prescriptions was somewhat limited. All the foreign prescriptions he had seen were written with great clearness and in a perfectly straightforward way. He (the lecturer) would like to give them one example from his own experience, and this happened quite recently. The case was interesting because it showed the uncertainty that exists even in the best informed quarters. He had a prescription written by a very eminent surgeon and it called for sphero-cylindrical prisms. The prescription gave the number of degrees and the letters D and I or D and O occurred. In addition to this the prescriber went on to explain what was meant by these letters, and, further, added a line so that there should be no mistake regarding the true direction of the cylindrical axes. The prescriber also went to some length to explain the prism notation he was using, and he said the angle he had specified was the actual angle through which the light was deviated, and when he (the lecturer) searched the prescription for the bases of the prisms they had been omitted. He communicated with the optician who sent the prescription, and the optician thought that as the prescription was for reading the bases would be in, but Mr. Green said he was afraid his experience would not warrant his taking such a step. He communicated with the surgeon, and found that the bases of the prisms should be out, so it was a fortunate escape altogether.



Respecting the term "short oval," this was a normal oval made shorter by 1.5 mms., just as the long oval was lengthened by 1.5 mms. This increased, of course, the short axis of the lens and gave a greater vertical field. The short oval was commonly used, and the difficulty regarding these eyeshapes was not due to the fact that the round oval, which this Society recognised, had so many distinctive names, but it was the confusion between the round oval and the short oval. A great many prescribers when they wanted a short oval ordered a round oval, and he had sent them the round oval with the result that the lenses were returned, and in some cases this meant a considerable loss to the dispensing optician. It was no use arguing the matter out, because in a great many cases his experience taught him that the standards recommended by this Society were not known. What he would like to see was that the standards recommended by this Society should be put into suitable form with diagrams where necessary and sent to every optician throughout the country. It would be a step in the right direction, and would lessen their troubles infinitely.

With regard to reversible spectacles, he had said it was not possible to have reversible spectacles with cable sides and a fixed W bridge. He realised that it was possible with a W bridge that was not fixed, provided the sides were made to rotate also, but W bridges on reversible spectacles had generally to support a very great weight, and consequently, if they were made satisfactorily in the first case they were not likely to remain satisfactory. It was essential that such frames, on account of the weight they had to carry, should have a rigid front. He had overlooked the fact that a fixed W bridge could be used if the sides were made to rotate, but that did not do away with the difficulty regarding the height or angle of such a bridge. His point was, that if a patient insisted on cable side reversible spectacles then a fixed W bridge was out of court altogether.

As for G.F. spectacles and the length of time they lasted, he agreed with what had been said, but he did not think that they were logical in that matter. They would find on some G.F. spectacles a ticket stating that they were guaranteed to wear 10 or 15 years. He took that to mean that there was sufficient gold as a casing to last those years; but what was the use of it when the life of the frame was nothing like that length of time? With an exceptionally careful patient the frame might last for the former period, that was provided the pins were looked to occasionally, but with a person who subjected it to great wear it could not possibly run that length of time.

There was one other thing he would like to say about G.F. spectacles, and that was relating to the bridges. Spectacles were sometimes returned with the bridges in a bad state of corrosion. There were bitter words of complaint about the bridges, and the whole frame had been condemned as of inferior quality. When they considered the treatment meted out to this gold-filled alloy (which was simply brass forced into a seamless tube of gold and soldered there) during the process of manufacture, it was not to be wondered at that sometimes the bridges developed such a condition as had been described. The bridges were fashioned by hammering, and in some cases the casing of gold under such drastic treatment developed slight cracks, which were almost impossible to detect during the final stages. But directly the patient got into a state of transpiration the perspiration found its way into these cracks, corrosion of the metal set up and the gold peeled from the bridge. It was a mechanical defect, and on account of that mechanical defect the frame could not be condemned as of inferior quality.

Regarding the statement that had been advanced, viz., that the deep tint of the edge of a concave lens might be an advantage to minimise aberration, he thought that that was rather a drastic way of treating the case, because such lenses appeared nothing too beautiful during the time they were being worn. He quite realised what had been said regarding the lenticular concave lenses; he knew that the retail optician met with opposition from the patient when suggesting the use of such lenses, but if concave lenses of minimum weight were required that was the only course open to them, unless exceptionally small sizes were used. He had some lenses on the table (indicating from the specimens), which had a tint at the margin of something of the order of 12 with a tint of No. 1 at the centre. He did not know of anything that looked worse on a patient than high power concave lenses which were deeply coloured at the margin, and with no colour at the centre, and he was afraid that the majority of patients would refuse such lenses if made.

**Mr. Hughes** said there was practically no tint in the centre.

**Mr. Green** replied that the specimens on the table had been made as specimens only to show the influence of these deep curves on the coloured blank.

**The Chairman** asked if Mr. Green had not met with blanks of plain glass with a deep tint in the centre and gradually growing fainter at the edges.

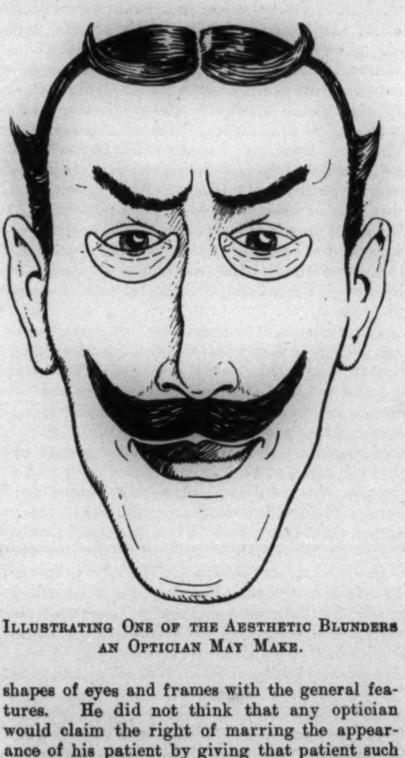
**Mr. Green** said he had never seen such a blank and thought that they would be extremely difficult and costly to make.

There was also the matter of coloured lenses, which was a very difficult question. There were recognised standard tints, but the dispensing optician could never be sure of getting coloured glasses, or, at any rate, smoked glasses of the same tint twice alike. The manufacture of this particular coloured glass seemed to be very difficult, and when they took into consideration the low cost at which this glass was produced they could not wonder that the manufacturers would not take steps to see that in every case the identical tint and colour was repeated. This was the difficulty in the matter of these glasses. Some of these smoked glasses were of a distinct green coloration, others brownish and even a distinct amethyst colour. He could not offer any authoritative word on the matter, but he did know that the greenish coloration in a mass of glass, due to the iron salts contained in the sand, was neutralised by means of manganese dioxide; possibly, when the greenish coloration existed in the smoked glasses neutralisation had not been completed. When the amethyst colour predominated then it might be that the glass had been overdosed with manganese dioxide. If the cost of these coloured lenses were increased many times, then it might be possible to obtain glasses of definite standard and definite colour, but under present circumstances the glass grinders had to be satisfied with what the manufacturer had to offer them.

**Mr. Hughes** said he had come across a case the other day where there was such a considerable difference that one would be quite justified in refusing them.

**Mr. Green** replied that considerable differences could, of course, be avoided, but in matching smoked lenses very great difficulty existed from the facts he had already pointed out. It often happened with globular spectacles especially. They received a globular spectacle containing one lens; and owing to this colour difficulty it was found, in many cases, impossible to match it. He had known cases where a whole day had been spent by a messenger trying to obtain a lens from other houses to match the one in the frame, and in the end two lenses had to be fitted to the frame because the existing one could not be matched. The value of the lens that should have been put in was a few pence only, and they would see from this that the coloured glass question involved the dispensing optician in many cases in considerable loss due to time alone.

There was another thing—he would not keep them much longer—and that was harmonising



Illustrating One of the Aesthetic Blunders an Optician May Make.

shapes of eyes and frames with the general features. He did not think that any optician would claim the right of marring the appearance of his patient by giving that patient such a shaped lens of frame that did not go well with the features, but he was afraid he would not be tempted to follow in all cases the lines of the features by some special type of eyeglass or eyeshape. It was an axiom in art that severe lines could not be beautiful, and if a patient with straight eyebrows, a thin straight mouth and a square jaw came to him he would feel it necessary to attempt to break up those straight lines by giving such a shape to the eyeglass and lens contrary to those severe lines of the features. This same question he remembered had cropped up once before in his experience, and there was one shape he would like to mention, and that was the crescent-shaped lens. (Describing.) They had only to imagine a man whose eyebrows curved upwards and who had a habit of turning the moustache up also, wearing such a pair of lenses. It needed only the central touch to complete the figure.

In conclusion, there was one other very serious matter he would like to submit to them, and that was regarding the use of white metal spectacles. If a patient had a tendency to eczema or any skin trouble a white metal frame would surely precipitate an attack, and he had seen some very dreadful cases of eczema set up through using these frames. These solid nickel frames—they could call them what they liked, hard nickel, white metal or any other fancy name, there was very little difference between them—contained about 53% of copper. The German white metal differed slightly from the American, and the bridge wire in both cases had a slightly different composition from the eyewire. The bridges of the German white metal frames had an approximate composition of 50% of copper, 11.12% of lead, 26% of nickel, and 18.88% of zinc, whereas the American white metal bridge wire contained approximately 54% of copper, 25% of nickel and 21% of zinc. It would be seen from these figures that they differed slightly in the amount of copper present, and it would also be noted that the

American wire contained no lead. The introduction of lead into white metal frames was undoubtedly to render it easier for working. These alloys contained neither antimony nor tin, so the skin trouble could not be attributed to antimony. If the skin was of an unhealthy character these eczema cases were bound to result, no doubt due to the large percentage of copper present, which the excretions of the skin would undoubtedly attack and some reaction would be set up. They used gold-lined bridges for the purpose of protecting the nose from the influence of these metals; but owing to competition, the gold on these bridges had been reduced to such an extent as to become practically useless, because the weight of the front displaced the tissue to such an extent that the white metal came in contact with the nose.

Similarly shell had been fitted to the bridges for the same purpose of protection, and yet they continued to pin these shells to the bridge with white metal pins. These pins, of course, should be of gold if pins were used at all. The sides would need attention also, or otherwise the same skin trouble would be set up behind the ears as on the nose. He, personally, had been threatened a great many times with all sorts of things because these alloys had set up these troubles, the patient attacking the optician and the optician, in his turn, attacking the wholesaler. Patients with such sensitive skin as had been mentioned ought to wear either gold-filled or gold frames.

# ON RECENT ADVANCES IN OPTICAL PROJECTION WORK.

### BY MR. R. S. WRIGHT.

*Lecture delivered on May 8th, 1913.*

MR. WRIGHT said that when he was first approached and asked to read a paper, he felt a little difficulty as to what he should bring before them, because there had been no such outstanding improvements in optical projection work in recent years as to warrant a full paper on that subject. He did not quite see what there was to talk about to fill in any great amount of time, but if they would allow him he would run over very briefly the position of optical projection work, and just glance at what had been done within the last few years and what could be done in optical projection work at the present time. If he had realised how extremely busy he would have been during the last few months he was afraid he would have refused the request.

The first thing he wanted to mention, perhaps the most important thing in many senses dealing with optical projection work, was the question of light. The ideal light for lantern work had of course not been discovered yet, but at the same time there had been various curious developments within the last few years in lights for optical projection work, and he would just mention that those amongst them whose experience in projection work went back for 20 years would remember that at that time there were to all intents and purposes only two forms of illuminants to be met with in lantern work.

Those two forms were the paraffin oil lamp and the oxy-hydrogen limelight; and it was hardly too much to say that 99 lanterns out of 100 were lit with one or the other. It was true that electric light had begun to make its way. There were a few lanterns here and there in institutions where they were lucky enough to possess current; and in that connection he had been rather amused to discover when he came into the room, that there was still in working order, lit with electric light, a lantern that, if he remembered rightly, he had had the pleasure of personally delivering there and demonstrating over 20 years ago; and he felt rather proud to think that it was still doing good service. At the same time, such a lantern as that was out of the common in those days. Electric lamps were much too heavy and somewhat clumsy. He had come across by accident that very day a sample of electric lamp which used to be in most common use in lantern work, the old Ediswan "focus" lamp, which he supposed they would remember gave 100 candle-power. Most of the currents met with were not more than 100 volts. It was rather curious as showing how things worked round in a circle that he had heard within the last two weeks that the Thomson Houston Co. had brought out a lamp for lantern work that was an improvement on the old Ediswan lamp. He had been trying to get hold of one but had not so far succeeded. The growth of electric currents had led to the growth in the use of electric lamps, and improvements in the way of the lesser radiants had led to the result that oil lamps had been done away with, and the lights that were not available 20 years ago had been gradually creeping in. He hoped they would excuse him for talking history, but it might be interesting to glance at the two or three forms of radiant that they could not get even a few years ago.

There were more acetylene lamps used now than anything else. He had on the table a very usual type of lantern acetylene lamp. There was nothing extraordinary about it, and he simply showed it as a general sample. As with everything else, they had had to learn from their faults. The development in acetylene generators had been rather to develop the gasometer principle. There had been two or three methods of making acetylene generators—the one known as the water-dropping principle, the gasometer principle, and what he might almost call the back-pressure principle. The gasometer principle had held its own, and the trend of recent years had been to make the gasometers taller and taller. The acetylene burner of today gave a fairly intense, fairly steady light, and was in use, at any rate, for the purpose of showing slides, in hundreds of moderate-sized halls. Of course, the great disadvantage was the want of concentration. There was light enough here (demonstrating) to do any work in reason, but it was difficult to concentrate. Of course its cheapness was one of its advantages.

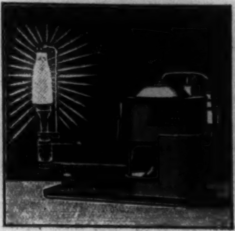


FIG. 1.

Another light which compared with the acetylene in intensity was the Welsbach spirit lamp (fig. 1). The Welsbach spirit burners were often used instead of acetylene, but they had their inconveniences. One was that it took some minutes to light up. The vaporised spirit had to pass down the channel to the Welsbach mantle. It had to be heated before it was lighted, and the spirit passed up into the mantle in the form of vapour. Care had to be taken at the commencement to see that the spirit was hot enough to readily vaporise, otherwise there might be an overflow of liquid, and perhaps burning spirit. He had known of at least one case where this actually did happen, due, he believed, to sheer carelessness on the part of the operator; but, although no harm had resulted it was perhaps as well to mention the point. It was one disadvantage of that form of lamp which had not yet been overcome, but its great convenience and portability were bringing it into favour.

He must mention arc lamps, which had been made increasingly smaller, more convenient and more powerful. There had not been any great developments in arc lamps lately except that there had been one or two fairly good automatic arc lamps placed on the market. The big lamps like the Oliver and the Siemens Schuckert, etc., they all knew were good, and gave a good steady light, but they wanted a very big lantern to put them in. With regard to hand-feed arc lamps, on the whole, the right-angled pattern had practically vindicated its claim to be recognised as the best. The one on the table (demonstrating) known as the "Intense" Lamp (fig. 2), had an electro-magnetic arrangement, which kept the arc in the front of the carbons, and greatly increased the steadiness and intensity of the light.

Another and smaller lamp was made by the Westminster Engineering Co. (fig. 3). It also had a small magnet which threw the arc to the front. Another point about this lamp was that the arc was practically enclosed in a glass cylinder, and although it was not in any sense of the word a vacuum arc lamp, yet during the first few minutes of burning it consumed the oxygen in the cylinder, the carbon then burning for a long time without feeding. The beauty of the lamp (demonstrating), if he might call it beauty, was that it took a very small current, and it would work from an ordinary lighting socket. The light was very steady, although there was a little tendency to flicker when it was first brought out, but the Westminster Engineering Co. had introduced two small air valves which had a considerable effect in steadying the light, and a lamp like that could be put into any socket, which was obviously a great advantage, although the light was not sufficient for a large hall.

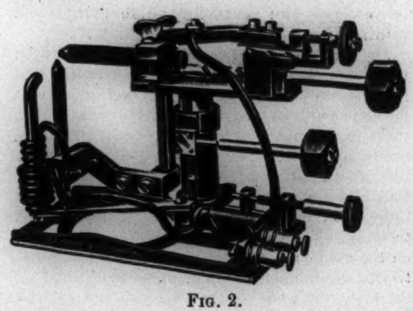


FIG. 2.

There was one form of electric light he had not mentioned, and that was the Nernst. That, of course, had been known for a good many years, but he did not consider it very suitable for lantern work. The light consisted of two or three rather long filaments, and there was a lack of concentration.

Having just glanced at those lights he would like to say that there was a very big want in optical projection work, and that was for a good intense lamp that was very portable. Of course they had, slowly making its way, the oxy-acetylene light; but unfortunately that was a very smoky sort of burner, very liable to choke and soot, and it involved the use of a cylinder of oxygen. Limelight was good enough for any ordinary purposes, but involved the use of compressed gas, which was a nuisance, and if anyone could discover a good light which did not necessitate the use of electric current or compressed gas cylinders, there would be a very great future for it. He had often wondered, although he had not had time to do any experimental work in that way, whether anything could be done with an acetylene jet worked with an air blast. Acetylene was an extremely hot blast, and although ordinary gas did not give sufficient intensity he had often thought that a good air gas in conjunction with an acetylene burner might solve a very great difficulty.

Having talked about the subject of lamps he would just like to spare a few minutes for a little glance in the opposite direction, a direction in which optical projection had gone back very much during the past few years. That was in the supply of projection lenses of good quality at a moderate price. Most of the optical houses 10 years ago could supply a good projection lens at prices from 30s. to 40s. He did not know where such a lens could be obtained to-day. They could go to one extreme of price and pay from five to twenty guineas, and they could go to the other extreme and buy projection lenses from 10s. to 16s., which latter were marvellous value for the money; but for the man who was prepared to pay, say, double that price, there was nothing so far as he knew, that was really good. He knew the reason, or thought he did. It was the enormous development of projection work in connection with cinematographs. Excellence of definition was not wanted, but cheapness was. Lantern lenses were turned out by the hundred and were marvellously good at the price; but manufacturers had given up, so far as he could trace, making the excellent lenses they used to make a few years ago at 30s. to 40s. For the last year or two he had been trying to find lenses at the price obtainable some years ago. That was a demand which still existed nearly as much as it did. A few years ago it was met; to-day it was not.

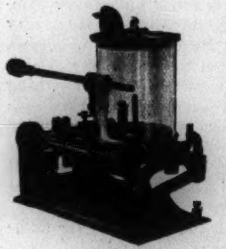


FIG. 3.

Having got so far as that he wanted to speak next about the development in optical lanterns themselves, and as there had been no developments worth speaking about for lanterns intended for slide demonstration only, he wanted to confine his remarks to lanterns for science purposes. There again there had been a rather curious development. There had been a great development of inexpensive science lanterns, and there had been also a great development of very expensive science lanterns, and a disappearance of science lanterns at moderate prices. He had a specimen or two with him of very inexpensive lanterns, and by the great kindness of Messrs. Leitz he was able to show them a specimen of an extremely expensive instrument; and nearly every technical college or institute nowadays seemed to go in for one or the other. That one (demonstrating) was a very simple thing, and a lantern of that sort was used throughout the country in hundreds. It was simply a lantern suitable for slide projection or vertical demonstration. A little mirror (demonstrating) threw anything which lay on a flat table on to the screen, and by just putting in another condenser lens the same lantern was equally good for ordinary slide work. The main point about it was the quickness by which the operator could change from one position to the other (fig. 4).

He still thought that half our schools had not realised the great advantage of a lantern with a vertical attachment. They had only to lay a piece of ground or smoked glass on the front—he had not got a piece with him—make any writing or drawing on it, and it appeared on the screen as it was executed. It was very much easier to do writing on a small table on a square piece of ground glass than to do it on a large blackboard. It was easy for the class to see it, and so far as he knew there was no disadvantage in using it. He believed that some day that mode of teaching would be more developed because lanterns of that kind were obtainable at a low cost—far more perfect than they were some years ago—and as schools and colleges began to realise the great advantage it would come in more and more. There were several lanterns similar to that, with the addition of an attachment for opaque projection showing scientific illustrations in body, and things of that sort. That made it a very handy apparatus indeed. Having thus very briefly shown a type of an ex-

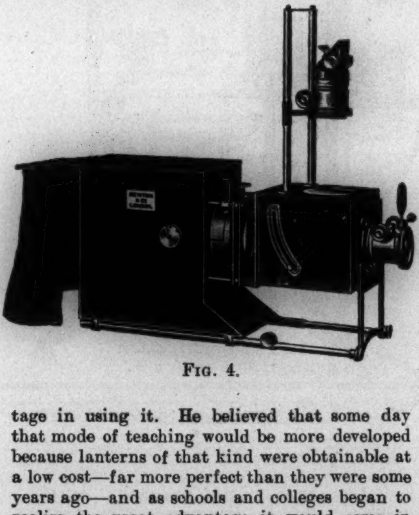


FIG. 4.

tremely inexpensive science lantern he would like to show them the magnificent piece of apparatus that Messrs. Leitz had kindly lent them (fig. 5).

The great peculiarity of that lantern was that it would throw on the screen anything from a live flea to an elephant! The lantern itself was simply a skeleton covered with a black curtain, and therefore every part was easily accessible. Another and very important point was the quickness with which each adjustment could be made. He would ask Mr. Ogilvy (of Messrs. Leitz), who was present, to just run through the points of the thing. It was extremely good for lantern slides, opaque work, or micro-projection. He was familiar with the lantern to a

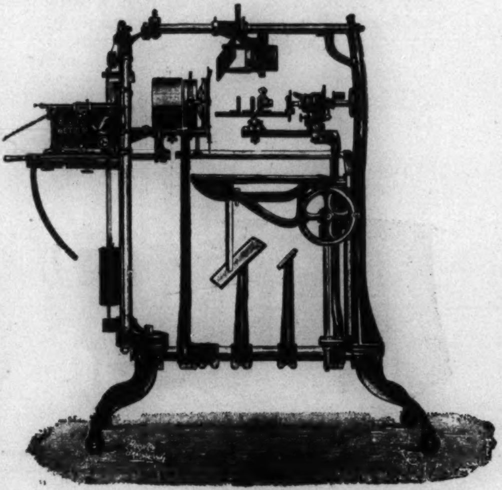


FIG. 5.

certain extent, but Mr. Ogilvy was more so. He thought it was a pity to give a lecture on optical projection and not show them something such as that, although it might be familiar to some of them. Mr. Ogilvy had kindly promised to bring his instruments, and he was very grateful to him for doing so.

[Mr. Ogilvy then gave a demonstration with the lantern referred to.]

There was another thing he would like to draw their attention to, and that was the subject of screens. There had been only one development in screens worth talking about during the last few years, and that had been in metallic surfaces. Metallic surfaces had had rather a curious history. Mr. Field, of Birmingham, first experimented with metallic screens in connection with his polarising lantern stereoscope.

Silver had the property of reflecting polarised light in the same plane of polarisation, and he projected two stereoscopic images on a silver screen by polarised light, and provided each of his audience with a pair of spectacles fitted with analysers. When a silver screen was used it was beautifully bright to anyone sitting immediately in front, but anyone sitting at the side lost more light than if the screen was made of the ordinary substance. The author's father (the late Mr. Lewis Wright) first improved on this by constructing the screen with a kind of striated silver leaf, which scattered the light sideways to a considerable extent. The thing lay dormant for some years until they now had screens consisting to all intents and purposes of aluminium paint, which had very much the same merit, and were, he thought, coming in more and more. They were very expensive, very short-lived, and they had a little of the old trouble, viz., that the greatest intensity of light was in the centre, and anyone sitting a little at the sides lost a little of the benefit. He had a small screen there, and they would see that there was a distinct gain in luminosity over a canvas surface; and where great light was wanted, as for example in high power micro-projection, there was certainly a future for the metallic screen.

He had nearly finished as regarded apparatus, but there were one or two minor developments in optical work he should like to speak about, and one was the development of autochrome work. He had a rather critical test to show them, as he did not think all of them realised how perfect autochrome work had become. His old colleague, Mr. T. E. Freshwater, had been making autochrome photographs of polarised light phenomena, and he had with him some of Mr. Freshwater's slides, which he would show on the screen side by side with the originals from which they were photographed, and which he would project in a large Nicol prism polariscope. They would see there was a good deal left to be done, although considering the obvious imperfections of the method it was surprising how much had been done.

[Mr. Wright then gave a demonstration of the above.]

Finally they would pardon him if he showed them something that was not exactly new, but he had been giving a good deal of time recently to improving it. It was a little apparatus for projecting Lissajous figures on the screen, and was originally designed by the late Mr. Lewis Wright, the author's father.

[Mr. Wright then gave a demonstration.]

The apparatus essentially consisted of a number of vibrating reeds set in motion by a bellows of peculiar construction, designed to give an extremely steady blast. A beam of light was projected from the lantern, first on to the mirror, then reflected to another one mounted on a second reed, and thence to the screen. A set of eight such mirrors were mounted on the same number of reeds forming an octave, and these reeds were arranged round a wooden cylinder on a rotating axis, by which any one could be brought into the beam of light. The second reed could also be changed if desired, but keeping any one note for this, a whole series of harmonies and discords could be produced on the screen at pleasure.

The first combination he showed them were two notes in unison, representing the lower C in fact, and when absolutely in tune the figure on the screen represented a circle ellipse or straight line, as the case might be. On setting the notes very slightly out the figure passed slowly through all these phases. The stationary figure was difficult to get, but could be obtained with patience.

The next one represented rather a bad discord. It would be represented on the piano by the notes C and D. The next one represented a difference of *two* tones (C and E), and gave them a harmony. When he reached the octave they would get the figure 8 going through its phases instead of a circle or ellipse. They would see as he changed the reeds that they were gradually approaching the figure 8, and finally the octave gave them the perfect figure (demonstrated).

As he had said before, if the remarks he had made were rather disjointed, it was because he had not had time to write out a paper as he would like to have done. In closing he would just like to say again that he wished the greater use of the lantern as a teaching instrument was more widely recognised. He believed there was an immense scope in school teaching and class teaching by the aid of the optical lantern, and although some of the scientific and technical schools were waking up to it he believed the time was not far distant when every school in the country worth the name would have its optical lantern.

**Dr. Rosenhain** said they had listened to Mr. Wright's lecture with considerable pleasure. The demonstrations he had given them, although not all of them new, were very beautiful and interesting, and he (the speaker) had been very pleased with them. He felt quite sure that if more of the members of the Society had known the kind of thing they were going to get that evening they would have made an effort to be present. There was one matter that Mr. Wright had touched upon that he would refer to, and that was the use of an acetylene-oxygen flame, with an air blast. If they used an air blast which would give a hot flame they would get a noise with it. They would get such a noise that they could not hear themselves speak, so that for optical projection purposes it was out of the question. There was another point which Mr. Wright did not mention, although it was very important. It was one of the great drawbacks of an air blast with acetylene. It made the lantern hot and was unpleasant to use. Another matter referred to was the use of the lantern for teaching purposes. There was one point which optical enthusiasts were apt to overlook. That was the difficulty of maintaining discipline in the dark. In the case of children the advantages derived from the use of optical projection from the teacher's point of view was more than counter-balanced by the amount of rope which was given to the more juvenile of the audience. The way out of the difficulty was to get a projection apparatus which was usable in a moderately lighted room. He supposed many of them had seen the daylight pictures; but that kind of installation was out of the question for the present. That brought them to the question of the metallic screen. If the screen were made of a number of vertical reeds, rotated rapidly, they would flicker the beam of light about to give a continuous impression to the eyes of the observers, and would probably distribute the light over the area required and obtain the advantages of the metallic screen without its disadvantages.

The CHAIRMAN (Mr. Chalmers) said he would like to express his indebtedness to Mr. Wright. He had been very much interested in seeing the method by which Mr. Wright had been able to show Lissajou's figures on the screen. He was also greatly interested in the comparison of those autochrome photographs. He had always been struck by the great difference there was between the results one obtained in scientific photography and landscape effects. One was disappointed with the former results. He had occasionally seen autochromes of illustrations in books, and they were not so satisfactory as one would like. It seemed to be reasonably easy to get good results in landscape work, but with autochrome photographs of more difficult subjects, it was very different. Better results might be obtained some day by a modification of the present plates and screens. Turning to the commercial side for a moment, he agreed with Mr. Wright that it was a mistake to make apparatus of the very cheap or the very expensive kind only. It was not everyone who could afford an apparatus which would do everything, and it was not everyone who wanted to do everything, but one did want to do well whatever he wanted to do. As a matter of policy he questioned whether it was wise to put into the same apparatus the projection of opaque and transparent objects, and of microscope slides.

Mr. Wright said with reference to the few remarks which had been passed, he would deal in the first place with the oxy-acetylene light. He was aware they could get a very good light indeed with an air-blast, but it was accompanied by a good deal of noise, as Dr. Rosenhain had remarked. The difficulty was to get a good light without noise, and he could not help thinking that it ought to be done. When they thought of the light they could get from a small bunsen burner or a spirit lamp, why they could not get a flame from acetylene without a hot air-blast he could not quite see. Acetylene, of course, was extremely dirty and very much inclined to soot and smoke anything it came into contact with, but it seemed to him that by experimental work those difficulties could be overcome. Of course, he knew that there were oxygen generators in the market which did away with the trouble of a cylinder. He had used those generators, and must frankly say that although if he were in the wilds of Africa he might be prevailed upon to use them, while he was in our happy island he would prefer to stick to the compressed gas cylinder.

With regard to water-cooled lanterns, the lantern Mr. Ogilvy had used that night was water-cooled. He believed that he used something in the neighbourhood of 40 amperes, and the heat would be sufficient to cook any slide that was not properly hardened. That necessitated water cooling, and modern lanterns were for that reason often fitted with water tanks. With regard to showing slides in daylight, it was only a question of light. A good arc lamp would show any ordinary lantern slide in good daylight. The one thing they must avoid was to have a beam of sunlight falling on the screen, and if they had a really powerful light they could see anything in reason without darkening the room.

With regard to the oscillating screen that had been mentioned, he was not convinced that this would be of any help. It was true they could see more or less of a sustained image by what was known as persistence of vision, but his impression was that they would lose more light in that way than they gained by the use of a silver screen. Many of them would remember the old experiment of the Newton's colour disc. It was a circular disc of card coated with sections giving all the colours of the rainbow, which when rotated showed white light. That was to say a white light in theory, but in practice anyone not familiar with optics would say that it was not white: it was grey. Grey, as they all knew, was simply absence of light. Each coloured section of the disc only reflected back a small portion of the light falling upon it, and instead of getting the effect of a white disc they lost some 4/5ths at least of the light, and the result was a grey disc. The lecturer always had to explain that it would be white if there was *enough* light, but as such a small portion of the light was reflected back it was not white but grey. He was rather afraid an oscillating metallic screen would have the same effect.

With regard to the apparatus for projecting Lissajou's figures. It was very simple. There were simply a number of vibrating reeds tuned to vibrate at certain regular rates, and therefore to give musical notes, although in practice they were almost too low to be called musical. They were rapid enough, however, to fulfil the scientific definition. The reeds were kept in vibration by an air blast, and at the lower end of each reed was a small mirror. A beam of light from a lantern was thrown on one mirror, from that to the mirror of the second reed, thence on to the screen. When they had two reeds in unison they get the oval, or circle, or the straight line, as the case might be. When the octave was in tune they got that figure 8, and all vibrations in between give either a simple or a complex figure according as two notes sounding formed a harmony or a discord. With regard to Mr. Chalmers' remarks, he rather agreed that there was room at the present time for a good opaque lantern on what he would call the aphengoscope principle, where a powerful beam of light was thrown on to a solid object and then that object was focused on to a screen. Many of them were familiar with the wonderful instrument known as the "Mirascope," and they had been very much struck by the brilliance of the illumination obtained by a couple of small incandescent gas lamps or electric lamps. What they wanted was a good lantern, not to sell at 2 or 3 guineas, but to sell at three or four times that price, and with a correspondingly more powerful light.

The Chairman asked if Mr. Wright had any knowledge of the new radiant which was being boomed so much in France, with intermittent electrical illumination; the inventor claimed that it would give very much more light than an ordinary lamp.

**Mr. Wright** said his only acquaintance with it was that he saw an article on the subject, and he had written to France about it. He was still waiting an answer.

The Chairman said he understood the matter had been before one of the French societies, and apparently it was regarded as worthy of serious consideration.

**Dr. Rosenhain** said a demonstration had been held in London the previous night.

# RECENT DEVELOPMENTS IN THERMOPILES AS APPLIED TO OPTICAL INSTRUMENTS.

### By Mr. L. Bellingham.

*Paper read 12th June, 1913.*

These few notes on thermopiles are based on the results of some experiments carried out during the past year with a view to obtaining some means of accurately measuring radiation in the infra-red spectrum, but at the same time avoiding the bolometer and its attendant difficulties.

It is comparatively easy to adapt a thermopile to an existing spectrometer, and at no great cost; and a suitable galvanometer is generally to be found in the equipment of a laboratory, while the bolometer is in itself more costly, special apparatus is required to obtain the best results, and in addition to this the bolometer is more difficult to control.

One of the first points to decide in designing a sensitive thermopile is, which materials will be most suitable for the junctions? On reference to tables, bismuth and antimony will at first appear to give the greatest electromotive force for a given rise in temperature, viz., 112 microvolts per degree C.; but as it is also necessary to produce the greatest rise of temperature at the junctions for a given radiation, the materials forming the junctions must be reasonably small and thin in order that the heat capacity may be low. Bismuth can be obtained in the form of wire in various sizes from 0.06 mm. up to 2.0 mms. in diameter, but up to now it has not been possible to produce antimony wire, so that a sensitive thermopile constructed of Bi Sb junctions is at present out of the question.

The material next in the list for use instead of antimony is iron, giving an E.M.F. against bismuth of 81 m.v. per degree C., but on account of its high electrical resistance, and great liability to rust, iron is passed over in favour of silver, which gives 72 m.v. per degree.

Thermopiles have been constructed of constantan (which is an alloy of 60% Cu and 40% Ni) and iron, but as the E.M.F. developed is only 50 m.v. per degree, up to the present bismuth and silver would seem to be the most suitable junction to use.

In a paper in the 'Annalen der Physik,' 1910, E. H. Johansen has given several important rules governing the construction of a sensitive thermopile of only one junction for use in vacuo, and it is on these lines that the thermopile about to be described is constructed.

Johansen has found that for a vacuum thermopile the loss of heat by conduction should be equal to that lost by radiation, and for a thermopile for use in air, that the conduction loss should be equal to the sum of the losses by radiation and convection, also that the resistance of the galvanometer must be equal to that of the thermo-element.

The first experiments were undertaken with a single junction to determine the percentage of the heat lost in the various ways. This junction consisted of a thin piece of silver foil with the two wires crossing near the centre, as recommended by Johansen. This silver foil receiver-plate was bent into the form of a V, as in the Féry single junction pile, the concave side being directed towards the source of radiation. By shaping the receiver in this way, the loss of heat by reflexion will be negligible.

The whole junction was then sealed up in a glass tube, suitable lead wires being provided for connection to a potentiometer. The tube was then exhausted to a pressure of .001 mm. of mercury, when the convection loss ceases. The junction was exposed to radiation from a

Nernst lamp at a suitable distance, and the E.M.F. measured on the potentiometer. Air was then admitted and a fresh measurement of the E.M.F. taken. From these measurements the rise of temperature of the junction was obtained, and knowing the lengths and diameters of the wires the heat lost by conduction can be calculated. One other reading was taken, this time with a known incident radiation, the junction being exposed to a Hefner standard lamp at a distance of one meter. The amount of radiation received by a surface of one square centimeter at a meter distance from a Hefner lamp is accurately known, and multiplying this by the area of the junction gives the energy received by that junction.

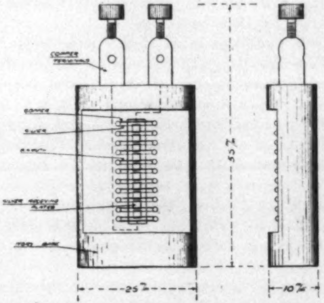
These readings give three equations, from which values for the convection and radiation losses are extracted. Having established the amount of heat lost in various ways, it is only a question of finding the best practical dimensions of the wires and sensitive area. The range of wires is naturally limited, and the most usual size of sensitive area for spectroscopic work is about 20 x 1.5 mms.; also on account of the very small E.M.F. available, it is desirable to keep the resistance of the thermopile as low as possible.

Taking all these points into consideration it was found that the most sensitive thermopile was one consisting of 10 junctions occupying a space of 20 x 1.5 mms., the diameters and lengths of wires being so chosen that they fulfilled as nearly as possible the conditions previously mentioned. (See illustration below.)

These junctions are mounted in a slot milled out in an ivory block, the wires being soldered to pegs placed on either side of the slot, and to the silver foil strips at the centre. It will be noted that there are really two "cold" junctions to each "hot" one, the two cold ones being connected in parallel with each other, but in series with the corresponding hot junction.

This arrangement is carried on throughout the thermopile, copper wires being soldered to the first and last pairs of pegs, and connected to terminals fixed in the top of the ivory block. The resistance of such a thermopile is only about 3 ohms, and it can be used with any sensitive low resistance galvanometer. The double set of cold junctions, coupled with their relatively large mass, prevents zero creep of the galvanometer to a great extent, while admitting of a rigid mechanical construction.

This troublesome zero creep is particularly noticeable with the familiar type of thermopile in which the cold junctions are arranged alternately and on each side of the hot junctions. The small mass of the cold junctions and their proximity to the hot ones, renders them liable to heat up by convection and conduction, even when carefully shielded from direct radiation, with the result that a reliable galvanometer reading is difficult to obtain. It is interesting to note that for a given sensitive area there is a correct number of junctions; and a thermopile constructed with twice that number of junctions is less sensitive than one with the correct number, although the resistance is still low.



There is one other junction which has been tried, viz., bismuth and tellurium; this gives an E.M.F. about eight times as great as bismuth-silver, but tellurium, owing to its crystalline structure, cannot be drawn into wire, and even were such wire obtainable, it could not be soldered, in fact thin copper and silver foil completely disappear when touched by heated tellurium. Nevertheless single bismuth-tellurium junctions have been constructed, and further experiments are in course of progress, but at present they are not of sufficient mechanical soundness, or permanence, to justify hopes that their great sensitiveness will be available in the near future for spectroscopic work.

MR. TWYMAN said the subject of the paper was one to which Mr. Bellingham had devoted a very great amount of work during the past nine months, and the series of experiments which he had briefly described, had taken a very long while to carry out. Those experiments had been most carefully done, and he was distinctly of opinion that Mr. Bellingham had arrived at a most efficient thermopile, composed of bismuth and silver, for use in air.

The CHAIRMAN said he very much appreciated the labour Mr. Bellingham had devoted to the subject, and the very great skill with which he must have carried out his experiments in order to obtain the results he had described. The construction of that type of thermopile would, he was sure, be of very great importance to English opticians, whose backwardness in this department was no doubt due to the fact that English instrument makers had not up to the present furnished the requisite apparatus. Of course, foreign instrument makers had the advantage that the initial work was done abroad, but he was very glad that one of our own instrument makers had now turned his attention to the matter, and that there was now a prospect that the work of spectroscopy would be developed in England as much as abroad. He hoped the enterprise of the maker would be well repaid, and that his apparatus would in many cases be used in preference to foreign ones.

# THE PRACTICAL MEASUREMENT OF COLOUR AND THE SENSITIVENESS OF THE EYE TO SMALL CHANGES IN HUE.

BY MR. F. STANLEY.

*Read June, 1913.*

ONE of the most difficult problems in the practical application of optics is the accurate analysis or specification of colour for the precision necessary in such that any colour may be specified within the limits of visual chromatic sensibility, and these, as we shall see later, are extremely small.

Many instruments have been designed with more or less success for the measurement of colour, but until we can obtain a light source more nearly approaching daylight, and more constant in character, the values obtained for colour standards will vary considerably. All artificial sources are deficient in violet compared with sunlight, and if we attempt to rectify this by employing a screen, the amount of light left is hardly sufficient to illuminate the apparatus, unless of course we employ the arc lamp, which has several disadvantages.

Existing colour measuring instruments can be arranged in four groups, *i.e.*—(1) Tricolour analysers, (2) Monochromatic analysers, (3) Interference instruments, (4) Instruments involving the use of comparison standards.

Of these groups, perhaps the tricolour analysers present the most difficulties, although under the trichromic theory of vision they should be the most reliable and satisfactory. In spectral instruments of this type the greatest difficulty lies in ensuring that the three entrance or illuminating slits receive the same amount of light. If sunlight is available it is not difficult to arrange this, but care must be taken to see that clouds are not focused on the slits.

Another difficulty comes in mixing the three beams after they have been dispersed by the prism. Several ingenious devices have been used for this, as for instance in the Ives colorimeter, where a rotating wheel is employed round the periphery on which are mounted a number of lenses, each lens deviating the light in two directions as the wheel revolves and thus effectively mixing the beams. The wheel must revolve at a sufficiently high speed to prevent flicker, which is in itself a source of possible error.

The three primary colours can also be mixed by using a series of half-silvered surfaces, but care must be taken to protect them, to prevent tarnish; and all three beams must suffer the same number of reflections. Tricolour analysers can also be used in which coloured screens take the place of the prism. Such instruments have the advantage of giving more light, and the screens are not very liable to change in time, although if very accurate measurements are desired the screens can be checked by a spectrophotometer.

A tricolour instrument in which only one entrance slit is employed was suggested several months back by Mr. F. Twyman, and a diagram is shown in fig. 1. The light entered through the slit A and passed through the optical train P, O, D, $R_1$, and $O_1$, to the concave mirror M. which was of exactly the same radius of curvature as the focal length of the object glass. The light was dispersed by the well known form of constant deviation prism D, and a spectrum in correct focus was formed on the surface of the concave mirror. The light forming the spectrum was allowed to return through three apertures placed in the correct positions relative to the spectrum, and the amount of light passing these apertures could be controlled by altering a series of neutral tint wedges.

Now as the concave mirror is of the same radius of curvature as the focal length of the object glass, the light passing the three apertures returns over exactly the same path as the incident light, and consequently would illuminate the back of the entrance slit and pass through it; but the reflecting cube P is composed of two right-angled prisms placed together with their

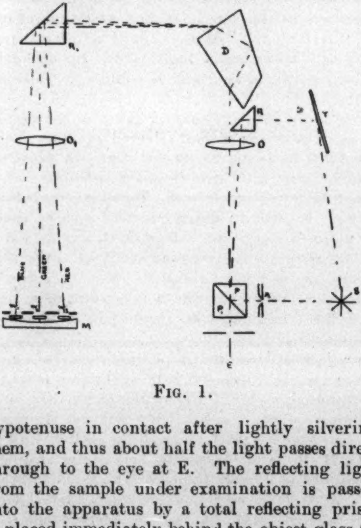


FIG. 1.

hypotenuse in contact after lightly silvering them, and thus about half the light passes direct through to the eye at E. The reflecting light from the sample under examination is passed into the apparatus by a total reflecting prism R placed immediately behind the object glass O. This instrument gave quite satisfactory results, and is possibly the most satisfactory form of spectral tricolour analysers.

Monochromatic analysers have only recently received much attention. Abney carried out exhaustive experiments in this direction, as did also König, but it remained to Prof. Nutting, of the Bureau of Standards in Washington, to design an efficient colorimeter on these lines. Colorimeters of this type depend on the fact that all colours with the exception of the purples can be formed from a definite spectral hue diluted with the necessary amount of white light. Purples can be matched by mixing a definite spectral hue with the purple to form white. The instrument designed by Prof. Nutting is exhibited here this evening; and it is proposed in America to base a legal standard of colour by the aid of such an instrument. The general design of the instrument can be best explained by the aid of fig. 2.

### The Nutting Colorimeter.

Light from the source S illuminates the three slits or apertures $S_1$ $S_2$ and $S_3$ by reflexion from three nickel-steel mirrors $M_1$ $M_2$ $M_3$. The light from the slit S is dispersed by the prism D, and a spectrum is formed by the lens $O_4$ at the position $S_4$, where it can be observed by the aid of an eyepiece.

The light from the slit $S_2$ is reflected by a glass plate or off the face of the prism, and an achromatic image of the slit $S_2$ is formed superimposed on the spectral image at $S_4$. The slit $S_3$ is the comparison slit, before which is placed the sample under test, and is placed almost perpendicular to the axis of the slit to prevent direct reflected light from passing into the slit.

The cube P is constructed from two right-angled prisms $P_1$ and $P_2$, one of which is silvered over half of the hypotenuse, and is so arranged that the light from the sample piece from $S_3$ fills this half of the cube.

In practice the eyepiece at $S_4$ is replaced by a pinhole, and the eye therefore observes direct the cube P. In front of each of the three slits $S_1$ $S_2$ and $S_3$ are arranged a pair of Nicols, so that either the spectrum colour white light or the light from the sample can be diminished to any desired extent and the amount of light can be determined.

The dispersing prism D is turned by means of the lever and a micrometer screw on the end of which is fitted a helical drum graduated to read wave-lengths direct. The instrument is

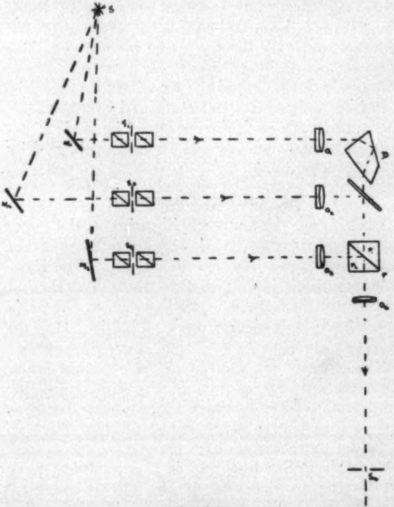


**Fig. 2.**

extremely easy to manipulate, and it is not difficult to arrange for all instruments to give the same results, which, of course, is of the greatest importance.

Of instruments employing the principles of interference of light the best one is that perfected by Arons, which although not in the final state is capable of fairly good measurements.

By this instrument colour is specified in terms of a thickness of quartz and on the angle between crossed Nicols. Six standard quartz plates are used and a white comparison surface is illuminated by the light passing the optical system.

When designing instruments for accurately recording colour values or arranging standard colour scales, it is necessary to remember the high sensitiveness of the eye to small changes in spectrum hue or wave-length. Being particularly interested in this subject some few years back, I designed a simple instrument for finding the smallest change in wave-length which can be detected by the eye under the most favourable conditions. The results obtained are interesting, and I afterwards found were in agreement with results obtained previously by other observers. A diagram of the instrument is shown in fig. 3.

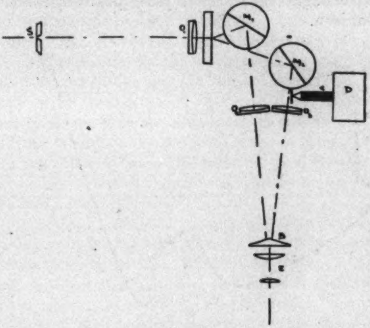


Fig. 3.

Light from the slit S is collimated by the lens O, and is diffracted by a grating, the orders on the right and left being reflected by the mirrors $M_1$ and $M_2$ through the lenses $O_2$ and $O_3$. A neutral tint glass of the required thickness is interposed in the beam from the brighter order to make the two beams of equal intensity. A biprism is placed at B to receive the two beams of light, and they are seen divided only by a fine line by the eyepiece E. The mirror $M_2$ can be rotated by means of a screw motion to which is fitted a helical drum calibrated directly in wave-lengths. The mirror $M_1$ can also be turned to bring on to the dividing line of the biprism light of any desired wave-length. With this instrument any desired portions of the spectrum can be brought into actual juxta-position for comparison.

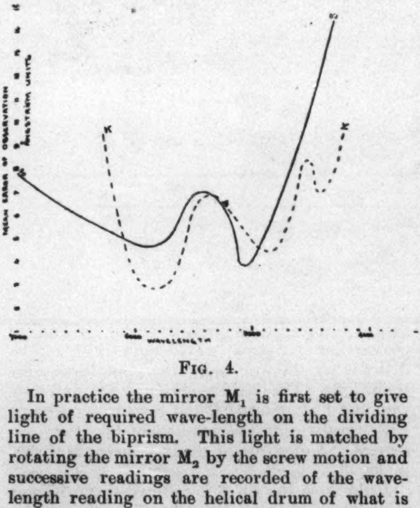


FIG. 4.

In practice the mirror $M_1$ is first set to give light of required wave-length on the dividing line of the biprism. This light is matched by rotating the mirror $M_2$ by the screw motion and successive readings are recorded of the wave-length reading on the helical drum of what is thought to be an exact match in colour on the two fields in the neighbourhood of the dividing line on the prism.

In the case of my own eye I took about 30 readings at intervals of about 50 units throughout the spectrum and the mean error of observation was recorded. If the values thus obtained are plotted, a curve can be drawn passing through most of the points. Such a curve is shown in diagram 4, and it will be noticed that there are two regions in the spectrum where the eye is most sensitive. These regions or maxima are at 5,050 and 5,890, the latter being at the position of the sodium lines. At wave-lengths 5,050 matches can be made to within three Angstrom units, or half the distance apart of the sodium lines. At 5,890 the eye is only slightly less sensitive, and matches can be estimated to within 4 or 5 units, slightly less than the distance of the sodium lines. This agrees exactly with results found by Lord Rayleigh and clearly shows that the difference in colour of the sodium lines can be detected, but as these lines are not of the same intensity it is difficult to detect the colour differences when using the ordinary spectrometer.

As will be seen, the results agree fairly well with those obtained by König, but his measurements were taken in a different manner. I have not, however, been able to detect with certainty the maxima at 4,500. These curves are of course of the greatest importance in detecting colour blindness, and they lend great support to the trichromic theory of vision for the maxima in the curve; 5,890 is where the red and green sensations of the eye overlaps, and at 5,050, where the blue, green and red sensations overlap.

This instrument is not entirely suitable for colour blindness tests, as a large number of readings are necessary to obtain consistent results, and for this reason the instrument designed by Dr. Edridge Green is more generally useful. With this instrument much more rapid determinations of the monochromatic patches can be made.

Mr. T. Smith said there were so many points of interest in Mr. Stanley's paper that he was afraid he could not cover them all, and must content himself with asking one or two questions of particular interest to himself. He had been very much struck by the ingenuity of the instruments Mr. Stanley had described. In the case of the one he had sketched on the board, he would like to ask whether he had experimented to determine whether he had noticed any different results by having the angle $M_3$ rotated in a different direction relatively to the slope of the sample. It was quite possible that one might get materials which reflected light differently—reflected some colours more strongly in some directions than in others—so that in order to obtain definite results it might be requisite to specify a particular standard of position for $M_3$. He very much admired the instrument described by Mr. Stanley as having been used for the mixing of the three colours together. He gathered that the new instrument was much quicker in use, and more satisfactory, but he did not quite understand whether such colours as browns could be matched by the mixture of one single monochromatic object in a white light. He would like to hear what could be done in that way. With regard to the apparatus for determining the sensitiveness of the eye to different colours, he did not know whether he had been setting his instrument to determine the accuracy with which colours could be repeated, or whether he had been doing what had been done by another investigator, namely, to make his error as large as possible without being able to say decidedly the difference in colour. He would expect the results obtained by making comparisons in these two ways to be very different indeed.

Mr. Everitt wished to know whether Mr. Stanley had noticed any change in the sensitiveness of one eye as compared with the other, because some years ago, when he had had occasion to make a series of spectral measurements, lasting over several months, at the end of that time he found that through using one eye at the red end, and the other at the blue or violet end, there was afterwards a distinct difference in the sensitiveness of the two eyes to colour, and, when looking at a white surface with the one eye, there was a slightly orange, or yellow, tint perceptible, while when using the other eye there was visible a bluish tint. Now, before making those observations, he had never noticed that difference, although it might have been there.

The CHAIRMAN said he was struck with the very great sensitiveness to colour differences shewn by Mr. Stanley's results. He would like to ask him how far he would consider that was due to the sort of differences obtained in reading Vernier readings, or whether he considered the effect would be much the same if one was observing only a comparatively small portion of each spectrum. He would have, he imagined, a comparatively complete spectrum on each field.

## The Lecturer's Reply.

MR. STANLEY: The sensitiveness of the eye determined in the manner described, is really the mean error of observation, the match in each series of measurements being made as carefully as possible. There is in the field of view two spectra, and any desired region from each can be brought into coincidence.

The CHAIRMAN: You would use the two spectra?

MR. STANLEY: Yes.

The CHAIRMAN: And you would not exclude any except just the actual portion you were dealing with? You would be able to use the whole of the rest of the spectra to aid your comparison?

MR. STANLEY: Yes, but cutting it down to about 10 to 20 units on either side made no difference to the results.

The CHAIRMAN: That is, with 10 or 20 units in each portion of the patch, you could set the two spectra against one another with an accuracy of about 5 or 10 units?

MR. STANLEY: Yes.

The CHAIRMAN: That is quite contradictory of the results published by Dr. Edridge-Green. If you could do it with such a small patch of the field, his results would not hold. You have two things along side one another, and he has them gradually changing from one to the other, and therefore the difference may be due to the fact that as the eye travels along you get a gradual change which is not appreciated, whereas, if you got even one-tenth of that change suddenly, you would appreciate it.

MR. STANLEY: The larger values obtained in the Edridge-Green instrument are, I think, due to the gradual change which is, as you say, not so well appreciated in finding a monochromatic patch. This, of course, is of no importance in practical colour blindness tests.

The CHAIRMAN: It would be very interesting indeed if Dr. Edridge-Green's experiments could be repeated with intervals—if he were to cut out pieces of the spectrum in between. I should imagine from your results it would be much easier to find colour changes under those circumstances than with a continuous spectrum.

MR. STANLEY: That might be the case, but, on the other hand, with his instrument it is much easier to tell whether anybody is colour blind than with mine. You can take readings much more quickly with his.

The CHAIRMAN said his reason for raising the point was that it affected the question how far Dr. Edridge Green's results were of value in deciding on colour vision. It might be that those results were dependent upon a special method of observation, and were not universally applicable to observations of limited portions of the spectrum.

MR. STANLEY: Dr. Edridge-Green's results are quite sound, and of the very greatest importance in colour blindness tests. The instrument described was used for determining only the sensitiveness of the eye to *small* changes in wavelength.

# PRACTICAL OPTICAL CALCULATIONS,

## WITH THEOREMS RELATING TO THE POSITIONS OF STOPS, AND EXACT FORMULÆ FOR THE CORRECTION OF CHROMATIC ABERRATIONS.

BY T. SMITH, B.A.,
A Senior Assistant in the National Physical Laboratory.

*Paper read November 13th, 1913.*

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§ 2. The functions of Geometrical Optics.
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§ 7. Formulæ relating to anallatic lens systems.
§ 8. The exact conditions for the correction of chromatic aberrations in any lens system.

### § 1. INTRODUCTORY.

THE purpose of this paper is twofold; firstly, to call attention to a method of arranging simple (first order) optical calculations; and secondly, to illustrate the advantages of this method by showing how they may be used to solve problems of importance with which the designer of a lens system is frequently faced. Some of these illustrations involve results which the author believes are new. There is nothing very novel in Sections 2, 3, and 4, but the fact that more complex methods of treatment are very widely used seems ample justification for placing this treatment of the Gauss theory at some length before this Society. The conventions adopted at the end of Section 2 appear to the author to simplify the theory, and they are of great importance when the relations of Section 5 are considered, and in the application of these relations to later sections. No apology is offered for the insertion of Section 3, though the substance of it must be familiar to all members of the Society. It contains the fundamental conventions and notation adopted throughout the paper, and appears to the author a desirable method of treating the subject when practical calculations, as distinct from a visualisation of the action of a lens system on light-waves, are the ultimate aim. Some objection may perhaps be made to the definitions of focal length, in that they have been defined as the distances of the principal foci from the nodal points rather than from the unit points. This corresponds to the Abbe rather than to the Gauss definition The author considers that by making this choice the distinction between focal length and back focal length is more readily realised. In practical cases the distinction is not important, with the possible exception of immersion objectives for the microscope; but in this case the important consideration is not the focal length, but the magnification with a given tube length, *i.e.*, it is the *position* of the image which matters rather than its absolute size.

Section 4 illustrates those which precede it. The important points are that calculations are much more simply made with powers than with focal lengths, and that they should be made first starting at one end of the system and afterwards starting at the other. When this is done the one calculation forms a check on the other, and all the quantities that can be wanted for any practical purpose are found in the course of the calculations.

The results given in Sections 5 to 8 are, so far as the author is aware, for the most part new. Those of Section 5 can be easily derived from the consideration of the properties of optical systems. Some of the results of Section 6 are chiefly of theoretical interest, but others have great practical importance. The simplest application is that shown in the paper—the placing of stops with a positive theoretical magnification. Section 7 contains some simple results of interest to the makers of anallatic telescopes. The formulæ of Section 8 are new, and enable the proper relative dispersions of any lens system to be determined for the correction of the various chromatic aberrations. All the simple cases usually given in books on the subject can be immediately derived from these formulæ. The quantities required for numerical computations have been determined in Section 4 in the course of finding the focal length.

## § 2. THE FUNCTIONS OF GEOMETRICAL OPTICS.

It is convenient to begin by considering the functions with which we are concerned in geometrical optics Suppose that we have a number of quantities

$$\kappa_1, \kappa_2, \kappa_3, \ldots \kappa_{n-1}, \kappa_n, \frac{t_1}{\mu_1}, \frac{t_2}{\mu_2}, \ldots \frac{t_{n-1}}{\mu_{n-1}}$$

Define the functions $K_{1,1}$; $K_{1,2}$; $K_{1,3}$; .... $K_{1,n}$ by the following equations :—

$$K_{1,1} = \kappa_1, \text{ so that } \frac{\partial K_{1,1}}{\partial \kappa_1} = 1;$$

$$\frac{\partial K_{1,2}}{\partial \kappa_2} = \frac{\partial K_{1,1}}{\partial \kappa_1} - \frac{t_1}{\mu_1} K_{1,1} = 1 - \frac{t_1}{\mu_1}\kappa_1;$$

$$K_{1,2} = K_{1,1} + \kappa_2 \frac{\partial K_{1,2}}{\partial \kappa_2} = \kappa_1 + \kappa_2 - \frac{t_1}{\mu_1}\kappa_1\kappa_2.$$

$$\frac{\partial K_{1,3}}{\partial \kappa_3} = \frac{\partial K_{1,2}}{\partial \kappa_2} - \frac{t_2}{\mu_2} K_{1,2}$$

$$= 1 - \left(\frac{t_1}{\mu_1} + \frac{t_2}{\mu_2}\right)\kappa_1 - \frac{t_2}{\mu_2}\kappa_2 + \frac{t_1}{\mu_1}\frac{t_2}{\mu_2}\kappa_1\kappa_2;$$

$$K_{1,3} = K_{1,2} + \kappa_3 \frac{\partial K_{1,3}}{\partial \kappa_3}$$

$$= \kappa_1 + \kappa_2 + \kappa_3 - \frac{t_1}{\mu_1}\kappa_1\kappa_2 - \left(\frac{t_1}{\mu_1} + \frac{t_2}{\mu_2}\right)\kappa_1\kappa_3$$

$$- \frac{t_2}{\mu_2}\kappa_2\kappa_3 + \frac{t_1}{\mu_1}\frac{t_2}{\mu_2}\kappa_1\kappa_2\kappa_3;$$

and so on, in general

$$\left.\begin{aligned} &\frac{\partial K_{1,p}}{\partial\kappa_p} = \frac{\partial K_{1,p-1}}{\partial\kappa_{p-1}} - \frac{t_{p-1}}{\mu_{p-1}}K_{1,p-1} \\ \text{and}\quad & \\ &K_{1,p}=K_{1,p-1}+\kappa_p\frac{\partial K_{1,p}}{\partial\kappa_p} \end{aligned}\right\} . \quad (1)$$

By proceeding in this way we finally arrive at the functions defined as $K_{1,n}$ and $\frac{\partial K_{1,n}}{\partial\kappa_n}$. It will be observed that the differential notation is justified, since the functions we are considering are linear in each variable, and the function we are calling $\frac{\partial K_{1,n}}{\partial\kappa_n}$ is, in fact, the coefficient of $\kappa_n$ in $K_{1,n}$.

Again, start at the other end of the system, and define the functions $K'_{n,n}$; $K'_{n-1,n}$; $K'_{n-2,n}$; . . . $K'_{1,n}$; by the equations

$$K'_{n,n}=\kappa_n, \text{ so that } \frac{\partial K'_{n,n}}{\partial\kappa_n}=1;$$

$$\frac{\partial K'_{n-1,n}}{\partial\kappa_{n-1}}=\frac{\partial K'_{n,n}}{\partial\kappa n}-\frac{t_{n-1}}{\mu_{n-1}}K'_{n,n}=1-\frac{t_{n-1}}{\mu_{n-1}}\kappa_n;$$

$$\begin{aligned} K'_{n-1,n}&=K'_{n,n}+\kappa_{n-1}\frac{\partial K'_{n-1,n}}{\partial\kappa_{n-1}} \\ &=\kappa_{n-1}+\kappa_n-\frac{t_{n-1}}{\mu_{n-1}}\kappa_{n-1}\kappa_n \end{aligned}$$

$$\begin{aligned} \frac{\partial K'_{n-2,n}}{\partial\kappa_{n-2}}=\frac{\partial K'_{n-1,n}}{\partial\kappa_{n-1}}-\frac{t_{n-2}}{\mu_{n-2}}K'_{n-1,n}&=1-\frac{t_{n-2}}{\mu_{n-2}}\kappa_{n-1} \\ &-\left(\frac{t_{n-2}}{\mu_{n-2}}+\frac{t_{n-1}}{\mu_{n-1}}\right)\kappa_n \\ &+\frac{t_{n-2}}{\mu_{n-2}}\frac{t_{n-1}}{\mu_{n-1}}\kappa_{n-1}\kappa_n; \end{aligned}$$

$$K'_{n-2,n}=K'_{n-1,n}+\kappa_{n-2}\frac{\partial K'_{n-2,n}}{\partial\kappa_{n-2}}=\kappa_{n-2}+\kappa_{n-1}+\kappa_n$$

$$-\frac{t_{n-2}}{\mu_{n-2}}\kappa_{n-2}\kappa_{n-1}-\left(\frac{t_{n-2}}{\mu_{n-2}}+\frac{t_{n-1}}{\mu_{n-1}}\right)\kappa_{n-2}\kappa_{n}$$

$$-\frac{t_{n-1}}{\mu_{n-1}}\kappa_{n-1}\kappa_{n}+\frac{t_{n-2}}{\mu_{n-2}}\frac{t_{n-1}}{\mu_{n-1}}\kappa_{n-2}\kappa_{n-1}\kappa_{n};$$

and so on, so that in general

$$\left.\begin{aligned}&\frac{\partial K'_{p,n}}{\partial\kappa_p}=\frac{\partial K'_{p+1,n}}{\partial\kappa_{p+1}}-\frac{t_p}{\mu_p}K'_{p+1,n}\\ \text{and}\quad &\\ &K'_{p,n}=K'_{p+1,n}+\kappa_p\frac{\partial K'_{p,n}}{\partial\kappa_p}\end{aligned}\right\}\ .\ .\ (2)$$

and by continuing the process right through the system we arrive at the function $K'_{1,n}$.

Now, if $n$ is put in turn equal to 1, 2, 3, &c., it will be seen from the values written out in full for these cases that for these values of $n$ we have

$$K_{1,n}=K'_{1,n},$$

and in fact it is not difficult to show that this is always true for all values of $n$. Therefore the accent in equations (2) is unnecessary, and we may in general write

$$\left.\begin{aligned}&K_{a,b}=K_{a,b-1}+\kappa_b\frac{\partial K_{a,b}}{\partial\kappa_b}=K_{a+1,b}+\kappa_a\frac{\partial K_{a,b}}{\partial\kappa_a}\\ &\frac{\partial K_{a,b}}{\partial\kappa_b}=\frac{\partial K_{a,b-1}}{\partial\kappa_{b-1}}-\frac{t_{b-1}}{\mu_{b-1}}K_{a,b-1}\\ &\frac{\partial K_{a,b}}{\partial\kappa_a}=\frac{\partial K_{a+1,b}}{\partial\kappa_{a+1}}-\frac{t_a}{\mu_a}K_{a+1,b}\\ &\text{where } K_{a,a}=\kappa_a \text{ and } \frac{\partial K_{a,a}}{\partial\kappa_a}=1\end{aligned}\right\}.\ (3)$$

It will be seen that for the equations (3) to be always true we must adopt the conventions

$$K_{a,b}=0\quad\text{and}\quad\frac{\partial K_{a,b}}{\partial\kappa_a}=\frac{\partial K_{a,b}}{\partial\kappa_b}=1$$

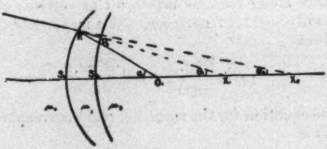
when $a=b+1$.

It must be remembered that the expressions $\frac{\partial K_{a,b}}{\partial \kappa_a}$, $\frac{\partial K_{a,b}}{\partial \kappa_b}$, and $\frac{\partial^2 K_{a,b}}{\partial \kappa_a \partial \kappa_b}$ merely denote the coefficients of $\kappa_a$, of $\kappa_b$, and of $\kappa_a \kappa_b$ in the function $K_{a,b}$, no matter by what process this has been obtained.

## § 3. REFRACTION THROUGH A LENS SYSTEM.

Turning now to a refracting system to see the application of these functions, let us consider a ray of light refracted at a spherical surface of radius of curvature $r_1$ (considered positive when the surface is convex to the incident light) which separates media of refractive indices $\mu_0$ and $\mu_1$.

Fig. 1.



Suppose that $O_1$ is the centre of curvature of the refracting surface and that the axis of the system meets the surface in $S_1$; let an incident ray which meets the axis at $X_0$ and the surface at $P_1$ be refracted to meet the axis at $X_1$. Then if the distance of $P_1$ from the axis be $h_1$, we have with the usual conventions of geometrical optics

$$h_1 = S_1O_1 \,.\, \alpha = S_1X_0 \,.\, \theta_0 = S_1X_1 \,.\, \theta_1,$$

where $\alpha$, $\theta_0$, and $\theta_1$ are the angles made by $P_1O_1$, $P_1X_0$, and $P_1X_1$ with the axis.

The equation of refraction gives

$$\mu_0(\alpha - \theta_0) = \mu_1(\alpha - \theta_1)$$

or

$$\mu_1\theta_1 = \kappa_1 h_1 + \mu_0\theta_0, \quad . \quad . \quad . \quad . \quad (4)$$

where

$$\kappa_1 = \frac{\mu_1 - \mu_0}{r_1}.$$

If the refracted ray meet a second surface in $P_2$ at height $h_2$, we have

$$h_2 = S_2X_1 \, . \, \theta_1 = (S_1X_1 - t_1)\theta_1,$$

where $S_2$ is the intersection of the axis with the second surface and $S_1S_2 = t_1$.

We then have

$$h_2 = h_1 - t_1\theta_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$= h_1 - \frac{t_1}{\mu_1}(\kappa_1 h_1 + \mu_0\theta_0) \quad \text{from (4)}$$

$$= h_1\left(1 - \frac{t_1}{\mu_1}\kappa_1\right) - \frac{t_1}{\mu_1}\mu_0\theta_0. \quad . \quad . \quad . \quad (6)$$

Now suppose that in general $r_p$ is the radius of curvature of the surface separating media $\mu_{p-1}$ and $\mu_p$, and that $\kappa_p = \frac{\mu_p - \mu_{p-1}}{r_p}$, also let $t_p$ be the distance along the axis between the vertices of the $p$th and $(p+1)$th surfaces. We may write (6) in the form

$$h_2 = h_1\frac{\partial K_{1,2}}{\partial\kappa_2} + \mu_0\theta_0\frac{\partial^2 K_{1,2}}{\partial\kappa_1\partial\kappa_2}. \quad . \quad . \quad . \quad (7)$$

The equation for the second surface corresponding to (4) is

$$\mu_2\theta_2 = \kappa_2 h_2 + \mu_1\theta_1,$$

and therefore from (4) and (7)

$$\mu_2\theta_2 = \kappa_2\left(h_1\frac{\partial K_{1,2}}{\partial\kappa_2} + \mu_0\theta_0\frac{\partial^2 K_{1,2}}{\partial\kappa_1\partial\kappa_2}\right) + \kappa_1 h_1 + \mu_0\theta_0$$

$$= \left(h_1 + \mu_0\theta_0\frac{\partial}{\partial\kappa_1}\right)\left(\kappa_1 + \kappa_2\frac{\partial K_{1,2}}{\partial\kappa_2}\right)$$

$$= h_1 K_{1,2} + \mu_0\theta_0\frac{\partial K_{1,2}}{\partial\kappa_1} \quad \text{by (1).}$$

By proceeding in this way we find finally

$$\mu_n\theta_n = h_1 K_{1,n} + \mu_0\theta_0\frac{\partial K_{1,n}}{\partial\kappa_1}, \quad . \quad . \quad . \quad . \quad (8)$$

and

$$h_n = h_1\frac{\partial K_{1,n}}{\partial\kappa_n} + \mu_0\theta_0\frac{\partial^2 K_{1,n}}{\partial\kappa_1\partial\kappa_n}, \quad . \quad . \quad . \quad (9)$$

$\kappa_m$ is called the power of the $m$th surface, and $K_{1,n}$ the power of the system of surfaces from 1 to $n$ inclusive.

If we start from the other end of the system we arrange the fundamental equations corresponding to (4) and (5) in the form

$$\mu_{p-1}\theta_{p-1}=\mu_p\theta_p-\kappa_p h_p,$$

and

$$h_{p-1}=h_p+t_{p-1}\theta_{p-1},$$

leading to

$$\mu_0\theta_0=\mu_n\theta_n\frac{\partial K_{1,n}}{\partial\kappa_n}-h_nK_{1,n} \quad . \quad . \quad (10)$$

and

$$h_1=h_n\frac{\partial K_{1,n}}{\partial\kappa_1}-\mu_n\theta_n\frac{\partial^2 K_{1,n}}{\partial\kappa_1\partial\kappa_n}. \quad . \quad . \quad (11)$$

Eliminate $h_1$ from equations (8) and (9) and obtain

$$\mu_n\theta_n\frac{\partial K_{1,n}}{\partial\kappa_n}-h_nK_{1,n}=\mu_0\theta_0\left(\frac{\partial K_{1,n}}{\partial\kappa_1}.\frac{\partial K_{1,n}}{\partial\kappa_n}-K_{1,n}\frac{\partial^2 K_{1,n}}{\partial\kappa_1\partial\kappa_n}\right).$$

Comparing this with (10) we see that

$$\frac{\partial K_{1,n}}{\partial\kappa_1}.\frac{\partial K_{1,n}}{\partial\kappa_n}-K_{1,n}\frac{\partial^2 K_{1,n}}{\partial\kappa_1\partial\kappa_n}=1, \quad . \quad (12)$$

a result which can be easily proved from the form of $K_{1,n}$. By means of the relation (12) if any two of the equations (8), (9), (10), and (11) are given. the other two can be found. In general in geometrical optics it is most convenient to use equations (8) and (10), as the coefficients $\frac{\partial K_{1,n}}{\partial\kappa_1}$ and $\frac{\partial K_{1,n}}{\partial\kappa_n}$ are necessarily found in calculating $K_{1,n}$ by the method of the preceding section.

Substitute in equation (8) for $h_1$ its value $S_1X_0 . \theta_0$: we then have

$$\mu_n\theta_n=\theta_0\left[S_1X_0.K_{1,n}+\mu_0\frac{\partial K_{1,n}}{\partial\kappa_1}\right]. \quad . \quad (13)$$

Thus if $F_0$ be a point given by the equation

$$S_1F_0=-\frac{\mu_0\frac{\partial K_{1,n}}{\partial\kappa_1}}{K_{1,n}},$$

the system has the property that all rays starting from $F_0$ emerge with $\theta_n=0$, *i. e.* parallel to the axis. Thus $F_0$ is the first principal focus.

If the final ray in general meets the axis in $X_n$, we have $h_n=S_nX_n\,.\,\theta_n$, and so from equation (10)

$$\mu_0\theta_0=\theta_n\left[\mu_n\frac{\partial K_{1,n}}{\partial\kappa_n}-S_nX_n\,.\,K_{1,n}\right]. \quad . \quad (14)$$

If $F_n$ is a point given by $S_nF_n=\dfrac{\mu_n\dfrac{\partial K_{1,n}}{\partial\kappa_n}}{K_{1,n}}$, all rays incident parallel to the axis, *i. e.* with $\theta_0=0$, will on emergence pass through $F_n$, which is called the second principal focus of the system. The length $F_0S_1$ is called the front focal length, and $S_nF_n$ the back focal length of the system. With these definitions of $F_0$ and $F_n$ equations (13) and (14) may be written

$$\mu_n\theta_n=F_0X_0\,.\,K_{1,n}\,.\,\theta_0$$

and

$$\mu_0\theta_0=-F_nX_n\,.\,K_{1,n}\,.\,\theta_n.$$

so that if the ray through $X_0$ passes after refraction through $X_n$, we shall have

$$F_0X_0\,.\,F_nX_n\,.\,K_{1,n}{}^2=-\mu_0\mu_n,$$

or if

$$F_0X_0=\frac{\mu_0}{N\,.\,K_{1,n}}, \quad . \quad . \quad . \quad (15)$$

we have

$$F_nX_n=-\frac{\mu_nN}{K_{1,n}}, \quad . \quad . \quad . \quad . \quad (16)$$

and

$$\mu_0\theta_0=N\mu_n\theta_n. \quad . \quad . \quad . \quad . \quad (17)$$

If $N=\mu_0/\mu_n$, from (17) we have $\theta_0=\theta_n$, the property defining nodal points; from (15) and (16) these are at distances $\dfrac{\mu_n}{K_{1,n}}$ and $\dfrac{\mu_0}{K_{1,n}}$ from the principal foci, or at distances

$$\frac{\mu_0\dfrac{\partial K_{1,n}}{\partial\kappa_1}-\mu_n}{K_{1,n}} \quad \text{and} \quad \frac{\mu_n\dfrac{\partial K_{1,n}}{\partial\kappa_n}-\mu_0}{K_{1,n}} \qquad (18)$$

from the first and last surfaces in the first and last media respectively. It will be shown below that one of the nodal points is with most systems virtual.

Again, suppose that the incident ray passes through the point whose coordinates referred to rectangular axes with $F_0$ as origin and the axis of $x$ along the axis of the system are $(x_0, y_0)$. We then have

$$h_1 = y_0 + x_0\theta_0 - \frac{\mu_0\theta_0 \dfrac{\partial K_{1,n}}{\partial \kappa_1}}{K_{1,n}} . \quad . \quad . \quad (19)$$

Similarly, if the refracted ray passes through $(x_n, y_n)$ when $F_n$ is the origin and the axes are in the same directions as for the incident rays, we have

$$h_n = y_n + x_n\theta_n + \frac{\mu_n\theta_n \dfrac{\partial K_{1,n}}{\partial \kappa_n}}{K_{1,n}} . \quad . \quad . \quad (20)$$

Now put

$$x_0 = \frac{\mu_0}{NK_{1,n}} \quad \text{and} \quad x_n = -\frac{\mu_n N}{K_{1,n}} . \quad . \quad (21)$$

Equations (8) and (10) then give

$$\mu_n\theta_n = y_0 K_{1,n} + \frac{\mu_0\theta_0}{N} \quad . \quad . \quad . \quad (22)$$

and

$$\mu_0\theta_0 = -y_n K_{1,n} + \mu_n\theta_n . N, \quad . \quad . \quad (23)$$

and therefore

$$y_n = Ny_0, \quad . \quad . \quad . \quad . \quad . \quad (24)$$

or N is the linear magnification for a small object placed perpendicular to the axis.

Putting $N=1$ in equations (21) we obtain the unit points, whose distances from the first and last surfaces are

$$\frac{\mu_0\left(\dfrac{\partial K_{1,n}}{\partial \kappa_1} - 1\right)}{K_{1,n}} \quad \text{and} \quad \frac{\mu_n\left(\dfrac{\partial K_{1,n}}{\partial \kappa_n} - 1\right)}{K_{1,n}}$$

respectively. One of these will in general be virtual as with the nodal points.

When $N=0$, equation (21) gives $x_0=\infty$ and equation (23) gives $\frac{y_n}{\theta_0} = -\frac{\mu_0}{K_{1,n}}$, *i. e.* $\frac{\mu_0}{K_{1,n}}$

o

measures the scale upon which an object at infinity in the first medium of refractive index $\mu_0$, which subtends a definite angle at the lens, is imaged in the medium of refractive index $\mu_n$. This quantity is called the second focal length or the focal length in the final medium. It is evidently the distance of the second principal focus from the second nodal point.

Similarly from equations (21) and (22) we have when $N=\infty$, $x_n=\infty$ and

$$\frac{y_0}{\theta_n}=\frac{\mu_n}{K_{1,n}}=\text{the first focal length}$$

= the distance between the first principal focus and the first nodal point.

It is important to observe that the front focal length and the first focal length are quite distinct, and similarly the back focal length and the second focal length are distinct: any similarity in the magnitude of these lengths is to be regarded as accidental.

Before concluding this section we may point out that from equations (17) and (24) we have

$$\mu_0\theta_0 y_0=\mu_n\theta_n y_n, \quad . \quad . \quad . \quad . \quad (25)$$

where $\theta_0$ is the inclination to the axis of a ray starting from the intersection of a small object of height $y_0$ with the axis in the medium of refractive index $\mu_0$, and $\theta_n$ is the inclination of the corresponding refracted ray in the medium of refractive index $\mu_n$ which meets the axis at its intersection with the image of the small object, the length of which image is $y_n$. This equation shows that $\mu_0\theta_0 y_0$ is unaltered by refraction. This result has very important practical consequences.

In the preceding pages it has been assumed that the ray lies in the plane of the paper in its passage through the lens system. It can easily be shown that the foregoing results hold equally well when paraxial rays not in a plane through the axis of the lens are considered.

## § 4. PRACTICAL CALCULATIONS.

All optical calculations should be made according to the scheme suggested by equations (1) and (2). The power of the system should be calculated both by starting at the first surface and by starting at the last surface. The agreement in the final result provides a good check on the accuracy of the work, and also all the intermediate quantities are useful in the ways shown in subsequent sections, as well as for calculating aberrations. The method to be followed will be made clear by taking a photographic lens as an example. The one given below is a Cooke Process Lens, the figures for which are given by Mr. Dennis Taylor on page 48 of his 'System of Applied Optics.' The signs have been altered to suit the conventions of this paper.

Radii of curvature

$$r_1=+1\cdot264\,;\; r_2=+1\cdot48\,;\;\; r_3=-2\cdot09\,;$$

$$r_4=-\cdot553\,;\;\; r_5=-\cdot5325\,;\; r_6=-2\cdot8.$$

Refractive indices for D line

$$=\mu_3=1\cdot6103\,;\; \mu_5=1\cdot524\,;\; \mu_0=\mu_2=\mu_4=\mu_6=1.$$

Thicknesses and separations

$$=\cdot105\,;\; t_2=\cdot232\,;\; t_3=\cdot358\,;\; t_4=\cdot0053\,;\; t_5=\cdot110.$$

Hence we have

$$\kappa_1=+\cdot482832 \qquad \kappa_2=-\cdot412365 \qquad \frac{t_1}{\mu_1}=\cdot065205$$

$$\kappa_3=-\cdot292010 \qquad \kappa_4=+1\cdot103617 \qquad \frac{t_3}{\mu_3}=\cdot222319$$

$$\kappa_5=-\cdot984037 \qquad \kappa_6=+\cdot187143 \qquad \frac{t_5}{\mu_5}=\cdot072178.$$

These calculations are given to six figures as they were made on a calculating machine, with which six figures are as easily obtained as five or four; it must not, however, be assumed that so many are required, nor, in the absence of some indication as to the accuracy with which radii of curvature etc. are given, that the last one or two digits have a definite meaning. It is, however, in some cases, such as in calculating the outstanding errors of well corrected systems, desirable to use about six figures, as we have to obtain for the measure of the residual aberration a quantity which is the difference of two large and very nearly equal numbers.

Starting at surface 1 and using equations (1) we have :—

| $p$ | $\frac{\partial K_{1,p}}{\partial \kappa_p}$ | $\kappa_p \frac{\partial K_{1,p}}{\partial \kappa_p}$ | $K_{1,p}$ | $-\frac{t_p}{\mu_p} K_{1,p}$ |
|---|---|---|---|---|
| 1... | 1 | ·482832 | ·482832 | −·031483 |
| 2... | ·968517 | −·399383 | ·083449 | −·019360 |
| 3... | ·949157 | −·277163 | −·193714 | +·043066 |
| 4... | ·992223 | 1·095034 | ·901320 | −·004777 |
| 5 .. | ·987446 | −·971683 | −·070363 | +·005079 |
| 6... | ·992525 | ·185744 | ·115381 | |

The first line is quite simple, for we have

$$\frac{\partial K_{1,1}}{\partial \kappa_1} = 1, \text{ and } \kappa_1 \frac{\partial K_{1,1}}{\partial \kappa_1} = K_{1,1} = \kappa_1 = \cdot 482832.$$

The number in the last column is found by multiplying this by

$$-\frac{t_1}{\mu_1} = -\cdot 065205:$$

thus $\quad \cdot 482832 \times (-\cdot 065205) = -\cdot 031483.$

The second line is started by adding

$$-\frac{t_1}{\mu_1} K_{1,1} \text{ to } \frac{\partial K_{1,1}}{\partial \kappa_1}, \textit{ i. e. } 1 - \cdot 031483 = \cdot 968517:$$

for the next column

$$-\cdot 412365 \times \cdot 968517 = -\cdot 399383:$$

and $\quad \cdot 482832 - \cdot 399383 = \cdot 083449:$

also $\quad -\cdot 083449 \times \cdot 232 = -\cdot 019360.$

Thus for the third line

$$\cdot 968517 - \cdot 019360 = \cdot 949157:$$

$$-\cdot 292010 \times \cdot 949157 = -\cdot 277163;$$

and $\quad \cdot 083449 - \cdot 277163 = -\cdot 193714;$

and so on.

We now have to go through the calculations from the other end of the system, according to equations (2). Here $n=6$.

| $p$ | $\frac{\partial K_{p,6}}{\partial \kappa_p}$ | $\kappa_p \frac{\partial K_{p,6}}{\partial \kappa_p}$ | $K_{p,6}$ | $-\frac{t_{p-1}}{\mu_{p-1}} K_{p,6}$ |
|---|---|---|---|---|
| 6... | 1 | ·187143 | ·187143 | −·013508 |
| 5... | ·986492 | −·970745 | −·783602 | +·004153 |
| 4... | ·990645 | 1·093293 | ·309691 | −·068850 |
| 3... | ·921795 | −·269173 | ·040518 | −·009400 |
| 2... | ·912395 | −·376240 | −·335722 | +·021891 |
| 1... | ·934286 | ·451103 | ·115381 | |

These calculations will be easily followed when the preceding set are understood. It will be observed that on both occasions we have reached the value ·115381 for $K_{1,n}$, and this agreement is good ground for the belief that the calculations are correct provided that the values obtained for the $\kappa$'s and $\frac{t}{\mu}$'s are correct in the first place.

From these figures we have

$$\text{Focal length} = \frac{1}{\cdot115381} = 8\cdot6669.$$

$$\text{Front Focal length} = \frac{\cdot934286}{\cdot115381} = 8\cdot0974.$$

$$\text{Back Focal length} = \frac{\cdot992525}{\cdot115381} = 8\cdot6022.$$

The value given by Mr. Dennis Taylor for the focal length is 8·6685, which differs from that obtained by the author; there is, however, agreement in the positions found for the principal points, viz. ·5695 and ·0647 from the first and last surfaces respectively. This discrepancy may possibly be caused by the comparative complexity of the method of calculation followed by Mr. Taylor. On comparing the two methods it will be found that the one advocated in this paper is at least as quick as the other, and that with it there is no trouble about signs.

## § 5. RELATIONS BETWEEN THE FUNCTIONS OF GEOMETRICAL OPTICS.

The equations of the preceding sections give complete information about the formation of images in the last medium of objects situated in the first medium. For the following sections, however, we require information about the rays in their passage between the first and last refracting surfaces, and this necessitates the application of some relations which will be obtained in this section between the powers and derived functions.

Suppose that $A, B, C, \ldots\ \alpha, \beta, \gamma, \ldots\ \rho, \sigma, \tau, \ldots$ are quantities independent of $p$, the Greek letters being used to designate the numbers by which individual surfaces of a lens system are identified. We suppose that $p$ is an integer greater than $\alpha, \beta, \gamma, \ldots$ but less than $\rho, \sigma, \tau, \ldots$ Suppose that for two consecutive values of $p$ the relation

$$\begin{aligned}0 = \mathrm{A}K_{\alpha,p}+\mathrm{B}K_{\beta,p}+\mathrm{C}K_{\gamma,p}+\ldots\\ +\mathrm{R}K_{p+1,\rho}+\mathrm{S}K_{p+1,\sigma}+\mathrm{T}K_{p+1,\tau}+\ldots\end{aligned} \quad (26)$$

is identically satisfied. Let these two values of $p$ be $p$ and $(p-1)$, so that we also have

$$\begin{aligned}0 = \mathrm{A}K_{\alpha,p-1}+\mathrm{B}K_{\beta,p-1}+\mathrm{C}K_{\gamma,p-1}+\ldots\\ +\mathrm{R}K_{p,\rho}+\mathrm{S}K_{p,\sigma}+\mathrm{T}K_{p,\tau}+\ldots\end{aligned} \quad (27)$$

Subtracting (27) from (26) and dividing throughout by $\kappa_p$ we have

$$\begin{aligned}0 = \mathrm{A}\frac{\partial K_{\alpha,p}}{\partial \kappa_p}+\mathrm{B}\frac{\partial K_{\beta,p}}{\partial \kappa_p}+\mathrm{C}\frac{\partial K_{\gamma,p}}{\partial \kappa_p}+\ldots\\ -\mathrm{R}\frac{\partial K_{p,\rho}}{\partial \kappa_p}-\mathrm{S}\frac{\partial K_{p,\sigma}}{\partial \kappa_p}-\mathrm{T}\frac{\partial K_{p,\tau}}{\partial \kappa_p}-\ldots\end{aligned} \quad (28)$$

Now multiply equation (26) by $1-\frac{t_p}{\mu_p}\kappa_{p+1}$, and equation (28) by $\kappa_{p+1}$ and add, and obtain

$$\begin{aligned}0 = \mathrm{A}K_{\alpha,p+1}+\mathrm{B}K_{\beta,p+1}+\mathrm{C}K_{\gamma,p+1}+\ldots\\ +\mathrm{R}K_{p+2,\rho}+\mathrm{S}K_{p+2,\sigma}+\mathrm{T}K_{p+2,\tau}+\ldots\end{aligned} \quad (29)$$

so that if the relation (26) is identically satisfied by two consecutive values of $p$ it will be satisfied by three consecutive values, and hence by induction it will be generally true.

This theorem may be used to prove that, adopting the conventions of equation (3), when

$$\alpha \leq \beta \leq \gamma \leq \delta,$$

$$K_{\alpha,\gamma} \,.\, K_{\beta+1,\delta} = K_{\alpha,\beta} \,.\, K_{\gamma+1,\delta} + K_{\alpha,\delta} K_{\beta+1,\gamma} \quad . \quad (30)$$

Since this is an identity, the coefficients of the various $\kappa$'s are identically equal, so that we also have

$$\frac{\partial K_{\alpha,\gamma}}{\partial \kappa_\alpha} K_{\beta+1,\delta} = \frac{\partial K_{\alpha,\beta}}{\partial \kappa_\alpha} K_{\gamma+1,\delta} + \frac{\partial K_{\alpha,\delta}}{\partial \kappa_\alpha} K_{\beta+1,\gamma} \quad (31)$$

$$\frac{\partial K_{\alpha,\gamma}}{\partial \kappa_\alpha} \frac{\partial K_{\beta+1,\delta}}{\partial \kappa_\delta} = \frac{\partial K_{\alpha,\beta}}{\partial \kappa_\alpha} \frac{\partial K_{\gamma+1,\delta}}{\partial \kappa_\delta} + K_{\beta+1,\gamma} \frac{\partial^2 K_{\alpha,\delta}}{\partial \kappa_\alpha \partial \kappa_\delta} \quad (32)$$

which is evidently a generalization of (12).

Again writing in equation (30) $(\beta-1)$ for $\beta$ and subtracting the result from the original equation we have on dividing by $\kappa_\beta$

$$K_{\alpha,\delta} \frac{\partial K_{\beta,\gamma}}{\partial \kappa_\beta} = K_{\alpha,\gamma} \frac{\partial K_{\beta,\delta}}{\partial \kappa_\beta} + K_{\gamma+1,\delta} \frac{\partial K_{\alpha,\beta}}{\partial \kappa_\beta}, \quad . \quad (33)$$

special cases of considerable importance being obtained by putting $\gamma=\beta$ or $\beta-1$. We also at once derive

$$K_{\alpha,\delta} \frac{\partial^2 K_{\beta,\gamma}}{\partial \kappa_\beta \partial \kappa_\gamma} = \frac{\partial K_{\alpha,\gamma}}{\partial \kappa_\gamma} \frac{\partial K_{\beta,\delta}}{\partial \kappa_\beta} - \frac{\partial K_{\alpha,\beta}}{\partial \kappa_\beta} \frac{\partial K_{\gamma,\delta}}{\partial \kappa_\gamma} \quad . \quad (34)$$

$$\frac{\partial K_{\alpha,\delta}}{\partial \kappa_\alpha} \frac{\partial K_{\beta,\gamma}}{\partial \kappa_\beta} = K_{\gamma+1,\delta} \frac{\partial^2 K_{\alpha,\beta}}{\partial \kappa_\alpha \partial \kappa_\beta} + \frac{\partial K_{\alpha,\gamma}}{\partial \kappa_\alpha} \frac{\partial K_{\beta,\delta}}{\partial \kappa_\beta} \quad (35)$$

and several other obvious relations.

## § 6. DETERMINATION OF THE POSITIONS FOR STOPS, ETC.

### (a) *The conditions under which a real non-inverted image can be formed.*

Suppose that an incident ray making an angle $\theta_0$ with the axis meets the 1st, $m$th, and $n$th surfaces at heights $h_1$, $h_m$, and $h_n$ respectively. We then have from equation (9)

$$h_m = h_1\frac{\partial K_{1,m}}{\partial\kappa_m} + \mu_0\theta_0\frac{\partial^2 K_{1,m}}{\partial\kappa_1\partial\kappa_m}$$

and

$$h_n = h_1\frac{\partial K_{1,n}}{\partial\kappa_n} + \mu_0\theta_0\frac{\partial^2 K_{1,n}}{\partial\kappa_1\partial\kappa_n},$$

or eliminating $\mu_0\theta_0$

$$\begin{aligned} h_m\frac{\partial^2 K_{1,n}}{\partial\kappa_1\partial\kappa_n} &= h_1\left(\frac{\partial K_{1,m}}{\partial\kappa_m}\frac{\partial^2 K_{1,n}}{\partial\kappa_1\partial\kappa_n} - \frac{\partial K_{1,n}}{\partial\kappa_n}\frac{\partial^2 K_{1,m}}{\partial\kappa_1\partial\kappa_m}\right) \\ &\qquad + h_n\frac{\partial^2 K_{1,m}}{\partial\kappa_1\partial\kappa_m} \\ &= h_1\frac{\partial^2 K_{m,n}}{\partial\kappa_m\partial\kappa_n} + h_n\frac{\partial^2 K_{1,m}}{\partial\kappa_1\partial\kappa_m} \end{aligned}$$

by equating the coefficients of $\kappa_\gamma$ in (32) when $a=\beta$.

This may be written in the form

$$\frac{\dfrac{\partial^2 K_{1,n}}{\partial\kappa_1\partial\kappa_n}}{h_1h_n} = \frac{\dfrac{\partial^2 K_{1.m}}{\partial\kappa_1\partial\kappa_m}}{h_1h_m} + \frac{\dfrac{\partial^2 K_{m,n}}{\partial\kappa_m\partial\kappa_n}}{h_mh_n} \quad . \quad . \quad (36)$$

a result which may evidently be at once extended to

$$\begin{aligned} -\frac{\dfrac{\partial^2 K_{1,n}}{\partial\kappa_1\partial\kappa_n}}{h_1h_n} &= \frac{t_1}{\mu_1h_1h_2} + \frac{t_2}{\mu_2h_2h_3} \\ &\quad + \frac{t_3}{\mu_3h_3h_4} + \dots + \frac{t_{n-1}}{\mu_{n-1}h_{n-1}h_n} \quad . \quad (37) \end{aligned}$$

showing that any lens system which has one paraxial ray meeting all the refracting surfaces on the same side of the axis has a negative value for

$$\frac{\partial^2 K_{1,n}}{\partial\kappa_1\partial\kappa_n}.$$

Now the distance from the first surface in the first medium at which an object must be placed for the linear magnification to be N is

$$\frac{\mu_0\left(\dfrac{\partial K_{1,n}}{\partial\kappa_1} - \dfrac{1}{N}\right)}{K_{1,n}} = \mu_0 a \text{ say.}$$

Also the distance from the last surface in the last medium at which the image will be formed is

$$\frac{\mu_n\left(\frac{\partial K_{1,n}}{\partial \kappa_n} - N\right)}{K_{1,n}} = \mu_n\beta \text{ say.}$$

For a real upright image to be formed it is necessary for both $\alpha$ and $\beta$ to be positive when N is positive. Thus, if the system is one of positive power, we must have

$$\frac{\partial K_{1,n}}{\partial \kappa_1} > \frac{1}{N} \quad \text{and} \quad \frac{\partial K_{1,n}}{\partial \kappa_n} > N$$

or $$1 + K_{1,n}\frac{\partial^2 K_{1,n}}{\partial \kappa_1 \partial \kappa_n} = \frac{\partial K_{1,n}}{\partial \kappa_1}\frac{\partial K_{1,n}}{\partial \kappa_n} > 1,$$

*i. e.*, $\frac{\partial^2 K_{1,n}}{\partial \kappa_1 \partial \kappa_n}$ must be positive.

Again, if the system is one of negative power, we must have

$$\frac{\partial K_{1,n}}{\partial \kappa_1} < \frac{1}{N} \quad \text{and} \quad \frac{\partial K_{1,n}}{\partial \kappa_n} < N$$

or $$1 + K_{1,n}\frac{\partial^2 K_{1,n}}{\partial \kappa_1 \partial \kappa_n} = \frac{\partial K_{1,n}}{\partial \kappa_1}\frac{\partial K_{1,n}}{\partial \kappa_n} < 1,$$

*i. e.*, $\frac{\partial^2 K_{1,n}}{\partial \kappa_1 \partial \kappa_n}$ must be positive.

For any lens, then, a necessary condition for the formation of a real non-inverted image is that $\frac{\partial^2 K_{1,n}}{\partial \kappa_1 \partial \kappa_n}$ should be positive: from (37) it follows that no real upright image is formed by any system which has any ray meeting all the surfaces on the same side of the axis. Since this necessary condition is not fulfilled in photographic lenses and many other types of lens, we conclude, on applying these results to the complete systems and also to any portions of them, that with such systems no real upright image of any object is formed wheresoever the object may be placed. It is important to observe that this applies to objects placed in the thickness of the glass or between component lenses.

From this result we draw the important conclusion that if a stop or other object is to be placed in such a position that the theoretical magnification of its image for the complete lens system is a positive quantity, there is only one position possible for it. This position may be in the first medium, in the last medium, or between the first and last surfaces. The position which it must occupy is determined by the need for correcting oblique aberrations, and it is of importance to be able to find readily where this position is when its theoretical magnification is given.

### (b) *The position of an object for a positive magnification.*

We have seen that if a small object is placed normal to the axis at a distance

$$\frac{\mu_0\left(\frac{\partial K_{1,n}}{\partial \kappa_1} - \frac{1}{N}\right)}{K_{1,n}}$$

in front of the first surface of the system of surfaces 1 to $n$ the linear magnification of the image will be N. The image of this object as formed in the first $m$ surfaces will be of magnification M where

$$\frac{\mu_0\left(\frac{\partial K_{1,n}}{\partial \kappa_1} - \frac{1}{N}\right)}{K_{1,n}} = \frac{\mu_0\left(\frac{\partial K_{1,m}}{\partial \kappa_1} - \frac{1}{M}\right)}{K_{1,m}},$$

*i. e.*,

$$\frac{K_{1,n}}{M} - \frac{K_{1,m}}{N} = K_{1,n}\frac{\partial K_{1,m}}{\partial \kappa_1} - K_{1,m}\frac{\partial K_{1,n}}{\partial \kappa_1}$$
$$= K_{m+1,n} \quad . \quad . \quad (38)$$

by equation (33).

If the height of the small object be $y_0$ and of its images in the $m$th and $n$th media be $y_m$ and $y_n$ respectively we have

$$y_0y_nK_{1,n} = y_0y_mK_{1,m} + y_my_nK_{m+1,n}, \quad . \quad (39)$$

a result which can at once be extended to

$$y_0y_nK_{1,n} = y_0y_1\kappa_1 + y_1y_2\kappa_2 + y_2y_3\kappa_3 + \dots$$
$$+ y_{n-1}y_n\kappa_n. \quad . \quad (40)$$

Other equations are evidently

$$\frac{K_{1,n}}{\mu_0\theta_0\mu_n\theta_n}=\frac{K_{1,m}}{\mu_0\theta_0\mu_m\theta_m}+\frac{K_{m+1,n}}{\mu_m\theta_m\mu_n\theta_n}$$

$$=\frac{\kappa_1}{\mu_0\theta_0\cdot\mu_1\theta_1}+\frac{\kappa_2}{\mu_1\theta_1\cdot\mu_2\theta_2}+\frac{\kappa_3}{\mu_2\theta_2\cdot\mu_3\theta_3}$$

$$+\ldots+\frac{\kappa_n}{\mu_{n-1}\theta_{n-1}\mu_n\theta_n}.\quad .\quad (41)$$

The distance $x_m$ beyond the $m$th surface at which the image will be formed is given by

$$\frac{x_m}{\mu_m}=\frac{\dfrac{\partial K_{1,m}}{\partial\kappa_m}-M}{K_{1,m}}=\frac{\dfrac{\partial K_{1,m}}{\partial\kappa_m}-N\dfrac{\partial K_{m,n}}{\partial\kappa_m}}{K_{1,m}+N\,K_{m+1,n}}\quad .\quad (42)$$

by equation (38),

$$\text{i.e.}\quad N=\frac{(t_m-x_m)\dfrac{\partial K_{1,m}}{\partial\kappa_m}+x_m\dfrac{\partial K_{1,m+1}}{\partial\kappa_{m+1}}}{(t_m-x_m)\dfrac{\partial K_{m,n}}{\partial\kappa_m}+x_m\dfrac{\partial K_{m+1,n}}{\partial\kappa_{m+1}}};\quad .\quad (43)$$

when $x_m=0$, we have

$$N=\frac{\dfrac{\partial K_{1,m}}{\partial\kappa_m}}{\dfrac{\partial K_{m,n}}{\partial\kappa_m}}.\quad .\quad .\quad .\quad .\quad .\quad (44)$$

Thus if we find for each surface of the system the value of $\dfrac{\dfrac{\partial K_{1,m}}{\partial\kappa_m}}{\dfrac{\partial K_{m,n}}{\partial\kappa_m}}$, we see at once between which surfaces a stop must be placed for its position to correspond with a given magnification for the whole system; for it must be placed between two surfaces $m$ and $(m+1)$, which have $\dfrac{\dfrac{\partial K_{1,m}}{\partial\kappa_m}}{\dfrac{\partial K_{m,n}}{\partial\kappa_m}}$ and $\dfrac{\dfrac{\partial K_{1,m+1}}{\partial\kappa_{m+1}}}{\dfrac{\partial K_{m+1,n}}{\partial\kappa_{m+1}}}$, the one greater and the other less than the required magnification.

The result obtained at the beginning of this section shows that the values for these ratios at the successive surfaces must all be either in ascending or in descending order of magnitude.

When the surfaces between which the stop must be placed have thus been found, the exact position between these surfaces is given by equation (42).

Taking the lens considered in Section 4, we find

for the values of $\dfrac{\;\dfrac{\partial K_{1,m}}{\partial \kappa_m}\;}{\;\dfrac{\partial K_{m,n}}{\partial \kappa_m}\;}$,

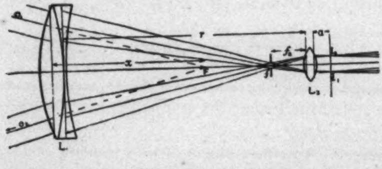
$$\begin{array}{ll}
m = 1 & \dfrac{1}{\cdot 934286} = 1{\cdot}0703 \\[2ex]
m = 2 & \dfrac{\cdot 968517}{\cdot 912395} = 1{\cdot}0615 \\[2ex]
m = 3 & \dfrac{\cdot 949157}{\cdot 921795} = 1{\cdot}0297 \\[2ex]
m = 4 & \dfrac{\cdot 992223}{\cdot 990645} = 1{\cdot}0016 \\[2ex]
m = 5 & \dfrac{\cdot 987446}{\cdot 986492} = 1{\cdot}0010 \\[2ex]
m = 6 & \qquad\qquad\quad \cdot 9925
\end{array}$$

Thus if the stop has to be placed in a position corresponding to a magnification greater than 1·0703 it will be in front of the first surface; between magnifications of 1·0615 and 1·0297 in the first air space, and between 1·0016 and 1·0010 in the second air space; below ·9925 it would be on the image side of the last surface. The intermediate magnifications 1·0703 to 1·0615, 1·0297 to 1·0016, and 1·0010 to ·9925 are not available, since they would require the stop to be in the substance of the glass.

## § 7. Anallatic Lens Systems.

Among the instruments in general use for surveying at the present time are telescopes, with stadia lines at a definite distance apart in the focal plane of the objective, which are used for the determination of the angle subtended by some object at the observer's station. With ordinary telescopes such angles are measured from the front nodal point of the lens, *i. e.* from a point near the surface of the lens. Thus, with the usual method of mounting the telescope the point at which the angle is measured moves whenever the telescope is pointed in a different direction. To avoid the necessity for correcting for this or to avoid the error from neglecting this factor, the so-called anallatic lenses are made, consisting of two lenses separated by an appreciable interval and with the point from which the angle is to be measured coincident with the point about which the telescope revolves. Such a system is shown in figure 2. $O_1$ and $O_2$ are two points on the object, and $I_1$ and $I_2$ their images as

Fig. 2.



formed by the complete lens consisting of the two separated components $L_1$ and $L_2$. The stadia lines meet the plane of the paper in $I_1$ and $I_2$. The angle corresponding to the separation $I_1 I_2$ will not vary with the distance of the object from the telescope if the rays defining the angle emerge parallel to the axis of the instrument; *i. e.*, the angles must be measured from F, the first principal focus of the complete lens, and this then must be at the fixed point about which the instrument moves. This focus will obviously be virtual, but there will be a real image of this virtual focus at the focus $f$ of the second lens $L_2$ if its focal length is suitably determined. It is important then to know what the focal lengths of the lenses must be to produce a given focal length for the combination, to bring the point F to a given position relative to the telescope tube, and to produce the image $I_1 I_2$ at a given place.

Denote the powers of the combinations $L_1$ and $L_2$ by $K_1$ and $K_2$, and of the complete lens by $K_{1,2}$.

From equation (33) we find that if T is the distance between the first unit point of the second lens and the second unit point of the first lens

$$K_{1,2} = K_1 + K_2 - TK_1K_2. \quad . \quad . \quad . \quad (45)$$

Also, if $x$ be the distance of F *behind* the first unit point of the first lens and $a$ the distance of $I_1 I_2$ for an object at infinity behind the second unit point of the second lens

$$x = \frac{-1 + K_2T}{K_{1,2}} \quad \text{and} \quad a = \frac{1 - K_1T}{K_{1,2}}. \quad . \quad (46)\&(47)$$

Multiplying together (46) & (47) we have by (45)

$$\frac{1}{K^2_{1,2}} - \frac{T}{K_{1,2}} + xa = 0, \quad . \quad . \quad . \quad (48)$$

or

$$\frac{1}{K_{1,2}} = F = \tfrac{1}{2}\left\{T \pm \sqrt{T^2 - 4xa}\right\} \quad . \quad (49)$$

where F is the focal length of the complete lens.

Now if $f_1$ and $f_2$ are the focal lengths of the lenses $L_1$ and $L_2$, we have from (47)

$$f_1 = \frac{1}{K_1} = \frac{T}{1 - aK_{1,2}} = \frac{TF}{F - a} = \frac{T(F - x)}{T - x - a} \text{ by } (48),$$

$$i.\ e. \quad f_1 = \frac{T(T - 2x \pm \sqrt{T^2 - 4xa})}{2(T - x - a)}, \quad . \quad . \quad . \quad (50)$$

and similarly from (46)

$$f_2 = \frac{1}{K_2} = \frac{T}{xK_{1,2}+1} = \frac{TF}{x+F} = \frac{T(F+a)}{T+x+a} \text{ by (48),}$$

$$\text{i. e. } f_2 = \frac{T(T+2a \pm \sqrt{T^2-4xa})}{2(T+x+a)} . \quad . \quad . \quad . \quad (51)$$

In equations (49), (50), and (51) it is of course necessary to use the same sign before the square root in all cases, and naturally the positive sign will be used.

As in practical cases the lenses used are approximately "thin," T will be very nearly the tube-length between the two lenses, $x$ the distance of the pivot from the front of the tube, and $a$ the distance from the second lens to the stadia lines. The above formulæ then give the focal lengths of the lenses in terms of the quantities most convenient for the maker to regard as his variables. Having found his focal lengths in this way, he can easily obtain a more exact result by taking account of the distances between the unit points and the outer surfaces for the focal lengths given by the first approximation.

## § 8. CHROMATIC ABERRATIONS.

From equation (33) we have

$$\begin{aligned} K_{1,n} = & \left(K_{1,m-1} + \kappa_m \frac{\partial K_{1,m}}{\partial \kappa_m}\right) \frac{\partial K_{m+1,n}}{\partial \kappa_{m+1}} \\ & + \left(K_{m+2,n} + \kappa_{m+1} \frac{\partial K_{m+1,n}}{\partial \kappa_{m+1}}\right) \frac{\partial K_{1,m}}{\partial \kappa_m} \\ & - \frac{t_m}{\mu_m} \left(K_{1,m-1} + \kappa_m \frac{\partial K_{1,m}}{\partial \kappa_m}\right) \\ & \times \left(K_{m+2,n} + \kappa_{m+1} \frac{\partial K_{m+1,n}}{\partial \kappa_{m+1}}\right), \end{aligned}$$

from which we see, if $\mu_{m-1} = \mu_{m+1} = 1$, that the coefficient of $\mu_m$ in $K_{1,n}$ is

$$\left\{ \frac{\kappa_m + \kappa_{m+1}}{\mu_m - 1} - \frac{t_m \kappa_m \kappa_{m+1}}{(\mu_m - 1)^2} \right\} \frac{\partial K_{1,m}}{\partial \kappa_m} \frac{\partial K_{m+1,n}}{\partial \kappa_{m+1}},$$

and that of $\frac{1}{\mu_m}$ is

$$-t_m\left\{K_{1,m-1}K_{m+2,n}-\frac{\kappa_m}{\mu_m-1}\frac{\partial K_{1,m}}{\partial\kappa_m}K_{m+2,n}\right.$$

$$-\frac{\kappa_{m+1}}{\mu_m-1}\frac{\partial K_{m+1,n}}{\partial\kappa_{m+1}}K_{1,m-1}$$

$$\left.+\frac{\kappa_m\kappa_{m+1}}{(\mu_m-1)^2}\frac{\partial K_{1,m}}{\partial\kappa_m}\frac{\partial K_{m+1,n}}{\partial\kappa_{m+1}}\right\};$$

these are the only two terms in $K_{1,n}$ involving $\mu_m$, and therefore

$$\frac{\partial K_{1,n}}{\partial\mu_m}$$

$$=\left\{\frac{\kappa_m+\kappa_{m+1}}{\mu_m-1}-\frac{t_m\kappa_m\kappa_{m+1}}{(\mu_m-1)^2}\right\}\frac{\partial K_{1,m}}{\partial\kappa_m}\frac{\partial K_{m+1,n}}{\partial\kappa_{m+1}}$$

$$+\frac{t_m}{\mu_m^2}\left\{K_{1,m-1}K_{n+2,n}-\frac{\kappa_m}{(\mu_m-1)}\frac{\partial K_{1,m}}{\partial\kappa_m}K_{m+2,n}\right.$$

$$\left.-\frac{\kappa_{m+1}}{\mu_m-1}\frac{\partial K_{m+1,n}}{\partial\kappa_{m+1}}K_{1,m-1}+\frac{\kappa_m\kappa_{m+1}}{(\mu_m-1)^2}\frac{\partial K_{1,m}}{\partial\kappa_m}\frac{\partial K_{m+1,n}}{\partial\kappa_{m+1}}\right\}$$

which at once reduces to

$$\frac{\partial K_{1,n}}{\partial\mu_m}$$

$$=\frac{1}{(\mu_m-1)}\left[K_{m,m+1}\frac{\partial K_{1,m}}{\partial\kappa_m}\frac{\partial K_{m+1,n}}{\partial\kappa_{m+1}}\right.$$

$$\left.-\frac{t_m}{\mu_m^2}K_{1,m}K_{m+1,n}+\frac{t_m}{\mu_m}K_{1,m-1}K_{m+2,n}\right]\quad\cdot\quad\cdot\quad(52)$$

Taking the coefficient of $\kappa_1$ on either side we obtain

$$(\mu_m-1)\frac{\partial^2 K_{1,n}}{\partial\mu_m\partial\kappa_1}$$

$$=K_{m,m+1}\frac{\partial^2 K_{1,m}}{\partial\kappa_1\partial\kappa_m}\frac{\partial K_{m+1,n}}{\partial\kappa_{m+1}}$$

$$-\frac{t_m}{\mu_m^2}\frac{\partial K_{1,m}}{\partial \kappa_1}K_{m+1,n}+\frac{t_m}{\mu_m}\frac{\partial K_{1,m-1}}{\partial \kappa_1}K_{m+2,n}\,. \quad (53)$$

Hence from (52) and (53)

$$(\mu_m-1)\left(K_{1,n}\frac{\partial^2 K_{1,n}}{\partial\mu_m\partial\kappa_1}-\frac{\partial K_{1,n}}{\partial\kappa_1}\frac{\partial K_{1,n}}{\partial\mu_m}\right)$$

$$=K_{m,m+1}\frac{\partial K_{m+1,n}}{\partial\kappa_{m+1}}\left(K_{1,n}\frac{\partial^2 K_{1,m}}{\partial\kappa_1\partial\kappa_m}-\frac{\partial K_{1,n}}{\partial\kappa_1}\frac{\partial K_{1,m}}{\partial\kappa_m}\right)$$

$$-\frac{t_m}{\mu_m^2}\left(K_{1,n}\frac{\partial K_{1,m}}{\partial\kappa_1}-\frac{\partial K_{1,n}}{\partial\kappa_1}K_{1,m}\right)K_{m+1,n}$$

$$+\frac{t_m}{\mu_m}\left(K_{1,n}\frac{\partial K_{1,m-1}}{\partial\kappa_1}-\frac{\partial K_{1,n}}{\partial\kappa_1}K_{1,m-1}\right)K_{m+2,n}$$

$$=-K_{m,m+1}\frac{\partial K_{m,n}}{\partial\kappa_m}\frac{\partial K_{m+1,n}}{\partial\kappa_{m+1}}$$

$$-\frac{t_m}{\mu_m^2}K_{m+1,n}^2+\frac{t_m}{\mu_m}K_{m,n}K_{m+2,n} \quad . \quad . \quad (54)$$

by equations (33) and (35).

Similarly we shall have

$$(\mu_m-1)\left(K_{1,n}\frac{\partial^2 K_{1,n}}{\partial\mu_m\partial\kappa_n}-\frac{\partial K_{1,n}}{\partial\kappa_n}\cdot\frac{\partial K_{1,n}}{\partial\mu_m}\right)$$

$$=-K_{m,m+1}\frac{\partial K_{1,m}}{\partial\kappa_m}\frac{\partial K_{1,m+1}}{\partial\kappa_{m+1}}$$

$$-\frac{t_m}{\mu_m^2}K_{1,m}^2+\frac{t_m}{\mu_m}K_{1,m+1}K_{1,m-1}. \quad . \quad . \quad (55)$$

Now any lens system may be regarded as a succession of lenses immersed in a medium of unit refractive index, for the power of a surface separating other media may be looked upon as the resultant power of two surfaces in contact, the one separating the one medium from air, and the other separating the other medium from air, taking the most usual case of the medium of unit refractive index. If, then, the preliminary calculations of the power of the system are made with such a treatment of cemented surfaces, we at once have,—

if $\varpi_m = \frac{\Delta\mu_m}{\mu_m - 1}$, $\Delta\mu_m$ being the increment in $\mu_m$ corresponding to a change in the wave-length of the incident light,—increment in focal length

$$= -\frac{\Delta K_{1,n}}{K_{1,n}^2}$$

$$= -\frac{1}{K_{1,n}^2}\Sigma\varpi_m\left[K_{m,m+1}\frac{\partial K_{1,m}}{\partial\kappa_m}\frac{\partial K_{m+1,n}}{\partial\kappa_{m+1}}\right.$$

$$\left.-\frac{t_m}{\mu_m^2}K_{1,m}K_{m+1,n}+\frac{t_m}{\mu_m}K_{1,m-1}K_{m+2,n}\right];$$

increment in front focal length

$$= \Delta\left(\frac{\frac{\partial K_{1,n}}{\partial\kappa_1}}{K_{1,n}}\right)$$

$$= -\frac{1}{K_{1,n}^2}\Sigma\varpi_m\left[K_{m,m+1}\frac{\partial K_{m,n}}{\partial\kappa_m}\frac{\partial K_{m+1,n}}{\partial\kappa_{m+1}}\right.$$

$$\left.+\frac{t_m}{\mu_m^2}K_{m+1,n}^2-\frac{t_m}{\mu_m}K_{m,n}K_{m+2,n}\right];$$

and increment in back focal length

$$= \Delta\left(\frac{\frac{\partial K_{1,n}}{\partial\kappa_n}}{K_{1,n}}\right)$$

$$= -\frac{1}{K_{1,n}^2}\Sigma\varpi_m\left[K_{m,m+1}\frac{\partial K_{1,m}}{\partial\kappa_m}\frac{\partial K_{1,m+1}}{\partial\kappa_{m+1}}\right.$$

$$\left.+\frac{t_m}{\mu_m^2}K_{1,m}^2-\frac{t_m}{\mu_m}K_{1,m+1}K_{1,m-1}\right].$$

If we have an object in the first medium placed normally to the axis of which the magnification for the standard wave-length of light is N, the increment in the magnification when the wave-length is altered will be

$$+NK_{1,n}\Delta\left(\frac{1}{K_{1,n}}\right)-N^2K_{1,n}\Delta\left(\frac{\frac{\partial K_{1,n}}{\partial\kappa_1}}{K_{1,n}}\right),$$

and the increment in the distance of the image formed by the light of new wave-length from the last surface of the lens over that for the original image will be

$$\Delta\left(\frac{\frac{\partial K_{1,n}}{\partial \kappa_n}}{K_{1,n}}\right)-2N\Delta\left(\frac{1}{K_{1,n}}\right)+N^2\Delta\left(\frac{\frac{\partial K_{1,n}}{\partial \kappa_1}}{K_{1,n}}\right).$$

Thus the conditions for complete chromatic correction at magnification N are

$$N=\Delta\left(\frac{\frac{\partial K_{1,n}}{\partial \kappa_n}}{K_{1,n}}\right)\Bigg/\Delta\left(\frac{1}{K_{1,n}}\right)=\Delta\left(\frac{1}{K_{1,n}}\right)\Bigg/\Delta\left(\frac{\frac{\partial K_{1,n}}{\partial \kappa_1}}{K_{1,n}}\right),$$

and for complete correction at all magnifications are

$$\Delta\left(\frac{\frac{\partial K_{1,n}}{\partial \kappa_1}}{K_{1,n}}\right)=\Delta\left(\frac{1}{K_{1,n}}\right)=\Delta\left(\frac{\frac{\partial K_{1,n}}{\partial \kappa_n}}{K_{1,n}}\right)=0,$$

and all these conditions are readily expressed numerically when the calculations for focal length, &c., are carried out in the method of this paper.

We may illustrate the calculations for chromatic correction by taking the figures for the Cooke Process Lens investigated in § 4. In the actual lens the same glass is used for the first and second lenses, but we may as well not make this assumption until our calculations are completed. Denoting the dispersive powers of the lenses by $\varpi_1$, $\varpi_2$, $\varpi_3$, we have

$$\begin{aligned}\Delta K_{1,n}=\varpi_1\Big[&{\cdot}083449\times{\cdot}912395\\&-\frac{{\cdot}065205}{1{\cdot}6103}\times{\cdot}482832\times(-{\cdot}335722)+0\Big]\\+\varpi_2\Big[&{\cdot}88325\overset{*}{3}\times{\cdot}949157\times{\cdot}990645\\&-\frac{{\cdot}222319}{1{\cdot}6103}(-{\cdot}193714)\times{\cdot}309691\\&+{\cdot}222319\times{\cdot}083449\times(-{\cdot}783602)\Big]\end{aligned}$$

---

[*] This is the power of the middle lens: all the other figures are found in Section 4.

$$\begin{aligned}&+\varpi_3\Big[-\cdot783602\times\cdot987446\\&\qquad-\frac{\cdot072178}{1\cdot524}\times(-\cdot070363)\times\cdot187143+0\Big]\end{aligned}$$

$$=\varpi_1\times\cdot082702+\varpi_2\times\cdot824247-\varpi_3\times\cdot773141$$

or increment in focal length

$$=-\varpi_1\times6\cdot18-\varpi_2\times61\cdot58+\varpi_3\times57\cdot77.$$

Again, increment in front focal length

$$\begin{aligned}=-74\cdot716\Big[&\varpi_1\Big\{\cdot083449\times\cdot912395\times\cdot934286\\&\qquad+\frac{\cdot065205}{1\cdot6103}(-\cdot335722)^2\\&\qquad-\cdot065205\times\cdot115381\times\cdot040518\Big\}\\&+\varpi_2\Big\{\cdot883253\times\cdot990645\times\cdot921795\\&\qquad+\frac{\cdot222319}{1\cdot6103}(\cdot309691)^2\\&\qquad-\cdot222319\times\cdot040518\times(-\cdot783602)\Big\}\\&+\varpi_3\Big\{-\cdot783602\times\cdot986492\\&\qquad+\frac{\cdot072178}{1\cdot524}\times(\cdot187143)^2-0\Big\}\Big]\end{aligned}$$

$$=-\varpi_1\times5\cdot33-\varpi_2\times61\cdot78+\varpi_3\times57\cdot63;$$

and increment in back focal length

$$\begin{aligned}=-74\cdot716\Big[&\varpi\Big\{\cdot083449\times\cdot968517\\&\qquad+\frac{\cdot065205}{1\cdot6103}\times(\cdot482832)^2-0\Big\}\\&+\varpi_2\Big\{\cdot883253\times\cdot949157\times\cdot992223\\&\qquad+\frac{\cdot222319}{1\cdot6103}(-\cdot193714)^2\\&\qquad-\cdot222319\times\cdot083449\times\cdot901320\Big\}\\&+\varpi_3\Big\{-\cdot783602\times\cdot992525\times\cdot987446\\&\qquad+\frac{\cdot072178}{1\cdot524}(-\cdot070363)^2\\&\qquad-\cdot072178\times\cdot115381\times\cdot901320\Big\}\Big]\end{aligned}$$

$$=-\varpi_1\times6\cdot74-\varpi_2\times61\cdot29+\varpi_3\times57\cdot92.$$

Now, taking the case when $\varpi_2=\varpi_1$, we find that the values of $\varpi_3/\varpi_1$ for the correction of

| | | |
|---|---|---|
| Focal length | should be | 1·1731, |
| Front focal length | „ | 1·1645, |
| Back focal length | „ | 1·1745. |

The Author desires in conclusion to thank Dr. Glazebrook, Director of the National Physical Laboratory, for looking through the paper and permitting it to be presented to the Optical Society.

## Discussion.

Mr. T. Y. Baker said: I have read with very great interest Mr. Smith's excellent paper and congratulate him upon the skill with which he has elucidated his various points

I have always found that anyone having a moderately extensive acquaintance with the more theoretical side of geometrical optics, is appalled at the laborious calculations that have to be undertaken as soon as he begins to apply his knowledge to practical cases, and that the very appearance of the mildest piece of calculation work, with the columns of figures arranged apparently in chaotic confusion, is sufficient to deter any mathematician from attempting to solve any of those optical problems that can be resolved by calculation work alone. Any effort, therefore, that aims at bringing the theoretical side of the subject more into touch with the practical is to be welcomed with open arms. But while I desire to thank Mr. Smith for his paper, I feel sure that he will not take it amiss if I offer a certain amount of destructive criticism.

In the first place, I do not altogether like the form in which he writes the preliminary equations in section 2, from which the power of the system and the focusing positions are determined.

It seems to me that they are much better expressed in a form which involves intercepts upon the surfaces, of a ray which starts in the first medium parallel to the axis. I will put down the equation for a system of coaxial thin lenses, because those are more familiar to me than those for the separate surfaces and of very similar form. If such a ray starts parallel to the axis and cuts the separate lenses at heights $l_1$, $l_2$, $l_3$ . . $l_n$ from the axis those quantities can be calculated from the equation:

$$\frac{l_2}{l_1} = 1 - d_1\kappa_1. \quad K_{1,2} = \kappa_1 + \frac{l_n}{l_1}\kappa_2.$$

$$\frac{l_3}{l_1} = \frac{l_2}{l_1} - d_2 K_{1,2}. \quad K_{1,3} = K_{1,2} + \frac{l_3}{l_1}\kappa_3.$$

$$\frac{l_n}{l_1} = \frac{l_n}{l_1} - d_n K_{1,n-1}. \quad K_{1,n} = K_{1,n-1} + \frac{l_n}{l_1}\kappa_n.$$

From these equations the equivalent power and the positions of the two foci can be readily obtained.

It will be seen that they agree with the author's equations, except that his partial differential co-efficients, $\frac{\delta K_{1,2}}{\delta K_2}$, etc., are replaced by $\frac{l_2}{l_1}$, etc., a course which seems to me to be much more reasonable as the ratios of intercepts have a definite physical significance, which is more than can be said for the pseudo-differential co-efficients that Mr. Smith uses. For they are not differentials; the system is perfectly fixed in every respect, and there is no question of variation in any of the powers of the separate surfaces.

I do not know whether the equations that I have written down have appeared anywhere in print. I myself obtained them from Mr. Chalmers, about five years ago, and I understand that he has used them for practical calculation a good deal longer than that.

Turning to the relations that appear in Sec. 5, 30, 31, 32, 33, 34, 35, I think the author is mistaken in supposing that they are new. If my memory serves me, these formulæ were given by Mr. Herman in his lectures on Geometrical Optics, in 1897. They are not, I believe, to be found in his book, but I think he elaborates them from some of the properties of continued fractions that he quoted from Chrystal's Algebra.

[Since the meeting I have looked the subject up. All these relations are particular cases of Euler's Theorem on Continuants. See Herman's Optics, paragraph 67, and Chrystal's Algebra, Vol. II., at the top of page 498.]

My last criticism is directed against the formulæ for the chromatic aberrations.

In a thin lens system the condition for achromatism for power is

$$\Sigma_1^n \kappa_r l_r l'_r \omega_r = 0.$$

Another condition for achromatisation for back focusing position

$$\Sigma_1^n \kappa_r l_r^2 \omega_r = 0.$$

and for front focusing position

$$\Sigma_1^n \kappa_r l_r'^2 \omega_r = 0.$$

where the accented letters are the intercepts upon the various lenses of a ray taken initially parallel to the axis, and traversing the system in the reverse sense.

These formulæ seem to me to be simpler than Mr. Smith's, and possess an advantage that one can see immediately to what extent a lens is contributing towards colour defects. For example, if either of the two rays intersects a lens at or near its centre, that lens will contribute a small term only in the power equation and in one of the focusing position equations, so that the glass could be varied considerably for the purpose of correcting the other focusing position.

Further, the equations for achromatic position of focus shew immediately that these conditions cannot possibly be obtained if positive lenses only are used.

I want also to comment on the extravagant accuracy the author has used in his calculations for achromatism. Even allowing for his statement that, worked on a calculating machine six figures gave no more trouble than four or five, I should like to point out that even four is excessive. The dispersive powers are given by the glass makers to three figures, and it is very doubtful whether the last figure is always reliable. To get an accuracy of four figures for the dispersive power C – F will entail measurement of the refractive index for these wave lengths to an accuracy of 1 in the fifth decimal place—which is an accuracy which does not obtain in the pieces of glass cut from a single melting.

Again, the chromatic corrections have not necessarily to be rigidly fulfilled—the example of a Ramsden eye-piece, which is good enough in its achromatisation for power, will illustrate the point—and to work these corrections with the accuracy to which Mr. Smith has gone, seems to me to be foolish and misleading.

In reply to Mr. T. Y. Baker, the author said that it was very valuable to have on record the notation preferred by those who had enjoyed the privilege of teaching optics for some years, and he appreciated the advantages obtained by using the ratios of intercepts instead of the differential co-efficients used in the paper. Some of the results given in Section 6, for instance, were fairly obvious once it was realised that the ratio of intercepts is equal to the differential co-efficient. Nevertheless, he was not prepared to abandon the differential notation in favour of that advocated by Mr. Baker. Given the function K the meaning of the differential coefficients used was obvious without explanation to anyone with a knowledge of fairly elementary mathematics. With Mr. Baker's notation, however, before the refraction of a general ray could be considered, it was necessary for him to show that the ratios of the intercepts for all rays incident parallel to the axis are approximately equal. The ratios he uses, too, are not, strictly speaking, the ratios for rays which meet the surfaces at finite distances from the axis, but the limits to which these ratio tend when the ray approaches the axis. Thus, to be exact. a differential notation should be employed.

Mr. Baker could not be taken seriously when he said that because the system is fixed the functions referred to are not really differentials. Could not a system be varied on paper, and was it not to paper systems that optical calculations were usually applied? Moreover, were there not concrete optical systems in which the power of individual surfaces was actually varied—for instance, an eye? Mr. Baker would seem to deny the right of differentiating in Pure Mathematics or of using the Calculus to determine the best dimensions for parts of an instrument to satisfy given conditions.

It appeared to the author that the differential notation was to be preferred in instructing all students who know the elements of the Calculus, and, in his opinion, those who do not should be advised to make it their first business, before proceeding with anything else, to acquire that knowledge. The Calculus-shy student was over much encouraged to persist in his shyness; and the substitutes offered to him were more troublesome to understand and less useful when acquired than the real article.

The author was much indebted to Mr. Baker for giving him a reference to the general theorem of which equations 30 to 35 are particular cases. He thought it would be very surprising if they were not known, but had seen no reference to them. They were of much importance in the more advanced parts of Geometrical Optics, but did not seem to be quoted in existing books.

He thought that Mr. Baker had entirely missed the point of the formulæ of section 8. Formulæ for the case of thin lenses were well known, and probably appeared in the text books; but so far as he knew formulæ, which take thicknesses into consideration, were not published. Mr. Baker's formulæ were exactly the same as the author's when $t$ was put equal to zero—they were neither simpler nor more complicated, but exactly the same, except that in the one case the ratio of intercepts was used for the differential notation of the other. The object of the section in question was to give the correct form of the additional terms which Mr. Baker left out of account. Mr. Baker suggested that his formulæ were sufficiently accurate: the author disputed this. In his opinion the actual thickness of lenses should always be taken into account: the fact that the thickness was small compared with the focal length or with the focal length of either component surface was not sufficient justification for neglecting it. Let them take, for instance, the case of a meniscus lens of refractive index 1.5, with its surfaces concentric: the thickness might be as small as they liked compared with the focal length, yet the true focal length was fifty per cent. greater than the value given by the thin lens formula. Meniscus lenses, too, it should be remembered, were of very frequent occurrence in combinations like photographic lenses, where the effect of neglecting a thickness would be very serious. Exact formulæ had for some time past been desired for calculations in such cases, and it was for that reason they were published in the paper.

The Ramsden eye-piece was brought forward in support of a suggestion that chromatic corrections might be very roughly made with satisfactory results. If this case were examined more carefully it would not be found to support this claim. It had to be remembered that an eye-piece was necessarily used with an eye, and that the latter was so insensitive that up to a magnifying power of at least ten a single lens was sufficiently achromatic for use as a magnifier. A Ramsden eye-piece of magnifying power, say fifty, would be satisfactorily corrected for colour if the chromatic differences of magnification were one-fifth of those for a single lens of the same focal length, *i.e.*, if the separation of the two lenses was four-fifths of the value required for complete correction, a result which agrees well with established practice. But an eye-piece was one of the very worst systems to take in deciding the accuracy required in formulæ for chromatic aberrations. For instruments that were to have a large aperture and large field of view, it was necessary to find the chromatic variations in the position of focus and in the focal length to a high order of accuracy. This could be done by using the formulæ of the paper.

As regards the criticism made of the number of figures used in the numerical illustrations, it may be said that these calculations were made primarily for the purpose of calculating aberrations other than chromatic, and for some of these it is necessary, in order to obtain a final result which has any meaning at all, to carry the intermediate figures to the extent shewn, even though the curvatures, etc., are not known to such an accuracy as to make the intermediate figures by themselves have any significance. The important thing is not necessarily to know exactly what the data are so much as in all the calculations to assume consistent, though possibly incorrect, values for them. A simple illustration will show the kind of thing which happens. It is required to find the value of $\tan x - \tan y$, where $x$ is known to be one degree greater than $y$, but $y$ is not known accurately—say, not nearer than about half a degree. It is known that $y$ is in the neighbourhood of ten degrees. From tables of tangents it is at once seen that the value of $\tan x - \tan y$ is 0.0181, and that this result is correct to about one in the last figure, even though the second figures in $\tan x$ and in $\tan y$ are uncertain. Exactly the same thing happens in finding residual aberrations in corrected optical systems: the final result, a difference of two comparatively large quantities, may be correct, though the intermediate quantities are wrong, so long as consistent assumptions—even if false—are made as to the exact values of the variables.

In putting down the figures used for chromatic calculations, it was thought desirable to quote exactly what appears in Section 4, so that no mistakes could arise as to the way in which any particular number was obtained. There was no intention of attaching a meaning to the last two figures in the values obtained for the ratio of dispersive powers required for the theoretical correction of the various chromatic aberrations. It was, however, considered that a knowledge of the magnitude of the differences between the three ratios in the case of a corrected lens was of sufficient interest to justify the addition of these figures. Such differences are likely to be practically constant, even when the refractive indices vary appreciably beyond the limits which would necessitate a recalculation of the entire system.

The CHAIRMAN (Mr. S. D. Chalmers, M.A.) said that he was much interested in Mr. Smith's paper, and Mr. Baker's remarks on it. He had not had an opportunity of going through Mr. Smith's methods. For teaching purposes, at least, he preferred his own notation, which did not involve differential symbols. He had recently written for publication an account of the methods he was using, and hoped that it would soon be available for those interested. Apart from the simpler notation his methods seemed to give simpler solutions to certain problems, particularly those relating to field of view. He would be glad to have the opportunity of comparing Mr. Smith's methods with his own.

As regarded chromatic aberration, the method which Mr. Baker had outlined did not take account of the effects of the thicknesses of the lenses. It was easy to include this additional term, and he presumed that the expression which he (Mr. Chalmers) used, consisting of terms of two types, would be algebraically equivalent to Mr. Smith's expression.

On behalf of the Society he desired to express the thanks of the meeting to Mr. Smith for his paper.